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Vol. I. STORAGE No. 7.

NATURAL SCIENCE:

A Monthly Review of Scientific Progress.

SEPTEMBER, 1892.

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NATURAL SCIENCE:

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NOTES AND COMMENTS.

The New Geology.

THAT the main features in the physiography of the earth's surface are not arranged haphazard, but in accordance with some definite, if unknown, system, was recognised at an early stage in the study of geography, and has become familiar to fourth-form schoolboys through the maxim that the peninsulas all point to the south. No serious effort, however, to formulate the law that governed the evolution of the Continents, or even to systematise the leading facts of the problem, was made till recently, with the exception of that of Elie de Beaumont; and this author's clock-face diagram of the orientation of mountain ranges has long since been dropped from geological text-books into the same category as the cycles and anti-cycles with which the Neo-Ptolemists bolstered up the astronomical system of their master. That Elie de Beaumont's speculations were of no practical value is not surprising, since insufficient data had been collected for the establishment of the laws that govern the denudation of highlands, the redeposition of sediments thus formed, and especially of the re-elevation of those sediments into plateaux and mountain chains. This has now been done and, as we have just been reminded, stratigraphers have been reduced during the past few years to filling in local details, though such work as Dr. Lawson's Report on Rainy Lake has shown that even in this branch of geology sensational results may still be achieved. The resting stage, however, has not been for long: a whole host of new and fascinating problems has been raised by Professor Suess' "Antlitz der Erde," a

work which promises to hold the same relation to the schemes of Elie de Beaumont that "the Origin of Species" held to the theories of Oken and Lamarck. During the past two or three years, the heresies of Suessism seem to have been steadily gaining ground, and some measure of their present influence may be estimated from the fact that they have formed the theme of two of the Presidential Addresses at the recent meeting of the British Association at Edinburgh.

The "New Geology," as Professor Lapworth has aptly named it, may be defined as the application of the results of the old geology to the discovery of the factors that have determined the remarkably symmetrical arrangement which, often disguised by local and superficial variations, rules in the existing physiography of the world. Its scope may best, perhaps, be comprehended by a perusal of the two addresses.

The main interest in Professor James Geikie's discourse to the Geographical Section on "Coast Lines" was, that he so clearly stated the differences between what Professor Suess has called Atlantic and Pacific types, the distribution of which has been figured by Neumayr.[1] Professor Geikie showed, moreover, that the cause of the difference was simply due to age: that round the great oceans, the main north and south coast lines probably date from the same primeval age, but that around the Pacific, and most typically on the American margin, comparatively recent earth movements have raised mighty mountain chains, now studded with volcanoes and closely following the extreme verge of the continental plateaux. On the Atlantic, on the other hand, the ranges upon the ocean border are of an enormous age, and therefore have been denuded into low rounded hills, are indented by gulf and fiord, and have been cut far back from the margin of the oceanic trough. In Professor Lapworth's address, on the other hand, the fundamental identity of the structure of oceans and continents was clearly demonstrated, the former being the troughs that balance the elevations of the latter: in each depression corresponds to depression, and ridge to ridge, and on both alike the lines of volcanic action lie along the weakened limbs of the great earth-fold. There is at first sight some apparent contradiction between the two addresses, but as far as such actually exists it is mainly on points of detail. At first it does seem as if the fact that the dominant north and south lines were developed in primeval antiquity were in favour of the permanence of the oceans and continents; but the mid-oceanic ridges probably date from the same time, and if amid the numerous relative changes of level they had stood above the ocean surface, and the existing continents had subsided, they would have furnished quite as strong an argument for their having been always continental, and existing land areas been always oceanic.

[1] Erdgeschichte, vol. i., p. 345.

The two addresses agree that the great mountain ranges of the world, whether above water or below them, had their foundations laid in the pre-geologic era of the solidifying globe, as the later crustal movements seem to have followed the original lines of weakness; and this not only applies to the great meridional chains, but to the intersecting east and west lines, such as the Mediterranean-Caribbean depression and the corresponding alpine line of elevation, which dates at least from Palæozoic times. Similarly they both conclude that the oceanic islets of the Pacific indicate an area of subsidence, and the submerged ridge of the land area there once cut up the Pacific Ocean, as the probable connection of the Atlas with the Venezuelan Cordillera once broke the continuity of the Atlantic.

It, therefore, seems probable that the new stratigraphers will allow the palæontologists the principal transoceanic bridges that they demand. Professor James Geikie suggests that the cosmopolitan distribution of Palæozoic species was really due to the absence of any oceanic abysses to bar the spread of littoral species. We thought it was now pretty generally understood that the supposed identity of, for example, the Carboniferous faunas of the world, was simply due to the diagrammatic figures of the European species having led to essential distinctions being ignored and species wrongly identified; closer study has shown that such world-wide faunas are myths, and that we have the same cases of some Palæozoic faunas common to the two sides of the Atlantic, with succeeding ones that are as sharply differentiated as any of later date. The study of extinct faunas is, however, as yet in its infancy, and it seems not unlikely that one of the results of the new geology may be the ultimate separation of the zoologists from the palæontologists, the former studying species and the latter faunas.

The problems suggested by the work of the Vienna school of geology are of intense interest but great difficulty, and will require for their solution the help of all the three branches of stratigraphy, petrology, and palæontology. It forms a curious comment on the oft-made statement that the future of Science is with the specialist, that the man who has done so much to set the pace for the new geology has written on nearly every branch of geological science, and is equally at home in the details of the stratigraphy of the Alps, the Central Plains, or the Tertiary basins. He has dealt with nearly every group of animals that has left traces for palæontologists to decipher, from sponges to mammals, and from fish to foraminifera, and has advanced as greatly the classification of Brachiopods as of earthquakes. He, moreover, as the author of *Die Zukunft des Goldes*, has taken rank as one of the ablest champions of bimetallism, and, until he incurred the wrath of anti-Semitic prejudice by a noble appeal for justice to the Jews, he was one of the very foremost of the leaders of the Liberal party in the Austrian Reichstag.

The Exploration of Mount Milanji, Nyassa-land.

A set of papers full of incident and interest was laid before Parliament last June. They relate to the suppression of slave-raiding in Nyassa-land, and tell how Commissioner Johnston, with the help of a few English officers and a handful of Indian sepoys, has been able to check and, for a time, completely paralyse the slave trade along one of the great routes from the interior to the coast, that, namely, which passes across the southern extremity of Lake Nyassa.

Central African expeditions do not always result beneficially to science. Through the Press or from the platform we get a faint idea of treasures still to be found, but nothing more tangible. The story is often one of aimless wanderings and perpetual fighting with the native tribes. The present series of operations, however, forms a pleasant exception. Mr. Alexander Whyte, who is described as a Naturalist and Scientific Horticulturist, was sent out by the British South Africa Company to work under Commissioner Johnston's instructions and thoroughly investigate the Natural History of British Central Africa, and pages 15—19 of the report contain an account of his exploration of the mountain and district of Milanji. "Milanji," he tells us, "is an isolated range of, for the most part, precipitous mountains, the main mass forming a huge natural fortress of weather-worn precipices, or very steep rocky ascents, sparsely clothed with vegetation. Many of its gullies and ravines are well wooded, and in some of them fine examples of grand African virgin forest are met with." It is about one hundred miles S.S.E. of Lake Nyassa and fifty miles east of the Shiré River. The ascent from the south-east face led at first over steep, grassy hills, down precipitous gorges and across rocky streams, then, after a more difficult climb, an interesting wooded gorge was reached where rocks and tree-roots afforded better foothold. Here, too, the plants of the lower slope gave way to more temperate forms, such as brambles and well-known papilionaceous and composite types. Ferns increased in number, together with selaginellas, lycopods, mosses, and lichens. What seemed an endless ladder of roots and rocks was at last succeeded by a dense thicket of bamboo; next a barrier of precipitous cliffs was surmounted by the aid of tufts of tussock-grass, and, finally, an hour's climb up a steep, grassy glen brought the party to the crest of the highest ridge. The view and the climate fully repaid the climb. The air was cool and bracing. Four thousand feet below, the thermometer had stood at 106° F. in the shade, while here they revelled in a clear dry atmosphere of 60° F. "Looking westward, we saw mapped out beneath us the plateau or basin of Milanji, with its rolling hills of grassy sward, its clearly-defined belts of dark-green forest, and its numerous ravines and rivulets, all shaping their course towards the principal valley through which the Lutshenya, the

principal stream, flows." The main peaks of the mountain rise directly from this river valley, which runs parallel with the southern base; the height of one of the two peaks has been calculated at 9,300 feet above sea-level. "Across the table-land in the distance is the somewhat isolated and precipitous Tshambi range, which, with its own smaller plateau, is separated from the Milanji table-land by the rocky valley and gorge of the Likabula River. To the front and left we have a continuation of the rolling grassy hills which encircle the plateau and are capped with rugged cliffs of scarped granite and gneiss rocks." Two weeks were spent on the plateau, working from three sites separated by from five to seven miles.

The flora proved a most interesting one. The most striking botanical feature was a fine cypress-like Conifer, apparently a new species of *Widringtonia*. This, however, seems to be rapidly disappearing before the annual bush fires. It was deplorable to witness the devastating effects of these fires, which, in the dry months of August and Sepember, originating from the villages on the lower slopes, creep up the precipitous cliffs from tuft to tuft of grass till the plateau is reached. Spreading quickly over the grassy table-land, the destroyer makes its way along the edges of the forests, burning or scorching the outside trees and killing the seedlings, sometimes even penetrating the interior where hundreds of giant trees lay prostrate and piled on each other in all stages of destruction. One of these, by no means the largest, measured 140 feet in length, with a clear straight stem of 90 feet, and $5\frac{1}{2}$ feet in diameter at 6 feet from the base. We are glad to hear that Commissioner Johnston has taken steps for their preservation.

Tree-ferns attain a great size in the damp shady forests of the plateau, while favoured nooks showed a gorgeous display of white and yellow helichrysums, purple and blue orchids and irises, and snow-white anemones. Several thousand specimens were collected and dried.

The fauna of the plateau and mountain-top, and of the surrounding plains, proved as interesting as the flora. It was, of course, more difficult to obtain representative collections of animals, birds, &c., in so short a time. The birds found in the "terai" of the mountain were largely identical with the species of the Zomba district (Zomba, in the Shiré Highlands, is the place of residence of the Commissioner); but those collected on and above the plateau differed widely, and many quite new forms were met with. The almost total absence of raptorial birds on the mountain was very noticeable, but Mr. Whyte suggests an explanation in the comparative paucity of animal life at so great an elevation, and the abundance of prey on the surrounding extensive plains. The birds on the plateau are, with very few exceptions, arboreal in habit, finding a safe retreat amid the dense foliage of the forest. Very few were found on the grass-lands—a small, dark brown quail, a pipit, two grass warblers, a snipe (rare), and the ubiqui-

tous great-billed raven. Thrushes, bulbuls, flycatchers, warblers, finches, and honeybirds made the woods lively with their varied notes. Altogether about two hundred bird-skins were collected, some of which may be new to science or of interest in connection with geographical distribution.

Comparatively few mammals were found. Of beasts of prey, the leopard, spotted hyena, serval, and an ichneumon were seen, while four species of rat were caught on the "terai," and three on the plateau. An interesting little antelope—probably a *Neotragus*—is confined to the rocky ridges of the higher range; it proved too shy and wary for capture. In the distance "it looked about the size of a half-grown goat, of a dark brown colour, with a white patch under the tail." Lions, which are common on the plains, do not apparently frequent the plateau. Snakes, of a good many species, were found both on the plains and plateau.

The soil of the grass-lands, owing to the impoverishing effects of periodical fires, is unsuitable for cultivation, but admirably fitted for sheep or cattle runs, the prevailing grasses being more tender and nutritious than the strong, rank herbage of the plains. The climate and the well-watered valleys also favour stock-rearing. As regards the soil around the base, Mr. Whyte considers it well suited for cultivation of coffee, while cacao would do well in many of the sheltered ravines along the river courses. The land, generally, is very productive; some gardens were seen from which the natives had reaped three crops of Indian corn and sorghum within twelve months. The rainfall is probably only from 60 to 70 inches, but apparently better distributed than at Zomba and other ranges in the Shiré highlands, for during one year's experience there had not been a single month without rain. Tobacco and cotton could be grown to an unlimited extent on the plains.

Viewed as a sanatorium, Milanji, according to Mr. Whyte contrasts favourably with some of the Indian and Ceylon hill-stations. The year is fairly equally divided into wet and dry months. During the former—November to May—it must be as miserable as all tropical mountain sanatoria. The other six months are fine, clear, and bracing, the thermometer sometimes falling at night below freezing point in May, June, and July. Of course, the great drawback in this respect is the practical impossibility of reaching it, but Mr. Whyte considers that a road of fairly good gradient might be made *viâ* the Lutshenya Valley; and this would also serve for transport of the valuable cypress timber now rotting in the forests.

Naturalists will look forward with interest to the detailed report on the collections obtained by the expedition, now being investigated by the officers of the British Museum. The Botanical results will be communicated next session to the Linnean Society, and a Zoological Report will be contributed to the Zoological Society's Proceedings.

The Age of the Earth.

About the time when Sir Archibald Geikie's Presidential Address to the British Association was exciting interest in the ever-recurring question of the Age of the Earth, the Physical Society of Glasgow University issued an admirable address by Professor John Young on the same subject. Both authors approached the problem from the point of view of the Naturalist as opposed to that of the Physicist; and both alluded to the circumstance that, whereas the "mathematical mill" will satisfactorily "grind whatever is put into it," no certainty can be reached so long as the initial data are merely plausible assumptions. While, however, Sir Archibald Geikie finds it impossible to accept Lord Kelvin's low estimate of geological time, the Physicist's colleague at Glasgow points out that there are many possibilities of grave error in the biological and geological arguments which may, after all, account for the difference of the result based upon the two lines of reasoning. The enormous period of time required for the evolution of the present world of life according to current views of Biology is, Professor Young thinks, a hopeless exaggeration due to our ignorance of the production of new organisms by some mysterious sudden change of the old ones. Following Eimer, he especially cites the case of the existing Axolotl, which remains a gill-breather so long as it can live in water, but suddenly changes into the lung-breathing *Amblystoma* as soon as water is inaccessible. He also alludes to the crustacean *Artemia*, a denizen of brine springs, which undergoes specific, even generic variation, according to the salinity of the water. Indeed, the Professor goes still further and declares himself in favour of the heresy, that the same type of animal may be simultaneously evolved from a distinct set of ancestors in two remote regions, thus doing away with the necessity of assuming great periods of time for the accomplishment of certain migrations. The oft-quoted arguments of the geologist also fare badly in Professor Young's hands. He points out that the rate of denudation of a country depends so much upon varying conditions as to render even plausible results impossible; and when a geologist speaks of strata miles in thickness, the Professor adds a word of caution that we fear has not been sufficiently attended to by those who attempt to estimate the length of geological periods. Professor Young indicates the great difficulty of determining the contemporaneity of deposits formed under different conditions, and he thinks that the estimated thickness of many geological formations has been greatly exaggerated by piling contemporaneous lake deposits, shore deposits, and deep-water deposits one upon the other. Finally, we are warned that some of our cherished views as to the antiquity of man may be the unfortunate outcome of the independence of the several lines of research, which have never been carefully correlated: it may be that "the net result is the expression not of conviction but of mutual courtesy."

No doubt there is much truth in all these considerations, and it is well that a Naturalist who can speak with authority should occasionally challenge the dogmas that are prone otherwise, by frequent repetition, to gain currency as well-ascertained facts. With regard to the age of the earth in terms of years, we are bound to admit that the solution of the problem is almost as far off as ever; and if a solution should eventually be found, we are convinced that the Astronomer, and not the Geologist or Biologist, will have the satisfaction of its discovery.

The Study of Variations in Animals.

Serious warnings that our philosophy may be attempting to advance too rapidly are not only conspicuous in the latest discussions of such wide problems as those to which we have just alluded. Recent literature contains many new facts that are likely to revolutionise current views on several of the minor questions of organic evolution; and if Mr. W. Bateson's researches on the variation of multiple parts in animals prove to yield something more than a phantom as a result, Biologists must cease to believe in some of their most fundamental tenets. It may be a satisfaction to learn that the five-fingered limb of a lung-breather is not a direct modification of the primitive fin of a gill-breather, but has merely arisen by the independent segmentation of the same kind of tissue that gave rise to the fins. It may also be a satisfaction to the student of teeth in the mammalia to know that the various modifications really cannot be regarded as all derived from one fundamental fixed type. It is somewhat startling, however, to read Mr. Bateson's preliminary announcement of his conclusions (*Proc. Zool. Soc.*, 1892, p. 114), which are shortly to be elaborated in a special treatise, and the following paragraph is especially noteworthy:—

"The received view of homology supposes that a varying form is derived from the normal much as a man might make a wax model of the variety from a wax model of the type, by small additions to, and subtractions from, the several parts. This may, to our imaginations, seem perhaps the readiest way by which to make the varying form if we were asked to do it; but the natural process differs in one great essential from this. For in nature the body of the varying form has never *been* the body of its parent and is not formed by a plastic operation from it; but in each case the body of the offspring is made again from the beginning, just as if the wax model had gone back into the melting pot before the new model was begun."

Our present methods of Comparative Anatomy, according to Mr. Bateson, "though they are ingenious, logical, and orderly, are orderly because they are made without regard to the ways of Variation, which is arbitrary and capricious and follows no order that we have yet devised."

The Evolution of the Mammalia.

Comparative Anatomy, however, when tempered with the results of Embryology and Palæontology, cannot be misleading to any great extent; and, notwithstanding Mr. Bateson's conclusions, Naturalists will still retain confidence in such researches as those of the Professor of Phylogeny at Jena, to whose discoveries in the evolution of the Mammalia we have often alluded. Dr. Kükenthal has just issued (*Biol. Centralblatt*, vol. xii., pp. 400–413, 1892) another of his striking addresses, which, though partly based upon published papers, contains much that is well-expressed and novel. The dentition, as usual, is mainly employed to determine the stage of evolution; and it is pointed out that in the three groups of Theromorous Reptilia, Marsupials, and Placental Mammals, the teeth become independently specialised in the same directions. In conclusion, the Professor believes that none of the known mammals can have been directly descended from any of the known Theromorous reptiles; he considers, on the other hand, that the ancestors of both these groups must be sought in the Palæozoic period.

Such a conclusion is interesting and perhaps justified, but at any rate it is provoking. The "ancestors" of certain well-marked groups of animals recede further and further into remote antiquity, just in proportion to the amount of research devoted to them. The reputed "ancestors" of one author become the "parallel line" of the next; and the geologist, who is supposed to unearth the desired pedigree, begins to despair of following a kind of will-o'-the-wisp. We cannot help thinking that someday a serious misapprehension will be discovered in the prevailing ideas of animal pedigrees. The "imperfection of the geological record" has its limits, and will not account for everything.

The Evolution of Man.

However unsuccessful the geologist may have been in unearthing the very earliest mammals, he has certainly done well in discovering the successive stages through which many groups have recently passed; and these processes of evolution have operated so rapidly in such modern times, that it is difficult to believe they have now entirely ceased. In many cases it is difficult to trace the directions in which they are advancing, on account of the paucity of material examined; but in the case of man, the innumerable variations in anatomy that are constantly being recorded afford a mass of very tangible evidence.

This evidence has lately been collected by Professor H. F. Osborn in the Cartwright Lectures for 1892, delivered before the Alumni of the College of Physicians and Surgeons of New York, and now printed in the *American Naturalist*, commencing June, 1892. It is certainly very striking when treated in Professor Osborn's philosophical

manner; and few will doubt that the human frame is still undergoing marked evolution.

In the first place, the Professor well points out that, physically speaking, man is far from being a pre-eminent creature:—

"From the typical mammalian standpoint, man is a degenerate animal; his senses are inferior in acuteness; his upright position, while giving him a superior aspect, entails many disadvantages, as recently enumerated by Clevenger, for the body is not fully adapted to it; his feet are not superior to those of many Lower Eocene plantigrades; his teeth are mechanically far inferior to those of the domestic cat. In fact, if an unbiased comparative anatomist should reach this planet from Mars he could only pass favourable comment upon the perfection of the hand and the massive brain! Holding these trumps, man has been and now is discarding many useful structures. I refer especially to civilised man, who is more prodigal with his inheritance than the savage. By virtue of the hand and brain he is, nevertheless, the best adapted and most cosmopolitan vertebrate."

Professor Osborn's review must be carefully read in detail to be appreciated. The lower jaw of man, we are told, is distinctly becoming reduced in size; the "wisdom tooth" is disappearing, and the life of civilisation is tending to a degeneracy of all the teeth. There is a frequent reduction in the number of the ribs, resulting in a shortening of the chest, probably correlated with man's upright position. The arm is becoming more adapted to freedom of motion. Owing to our present habits of walking, the great toe of the foot is increasing, while the little toe is diminishing; and in many human skeletons the two end-joints of the little toe are fused together. In many of the muscles, too, there is evidence of change; and there is no doubt that the man of the near future will differ much in his frame from the civilised man of the present.

The Ear.

Another welcome contribution reaches us from America in the form of an elaborate memoir on the organ of hearing in vertebrate animals (H. Ayers, *Journ. Morphology*, vol. vi., pp. 1–360, pls. i.–xii.). The subject is treated from the point of view both of Comparative Anatomy and Embryology; and if Dr. Ayers' results are confirmed, they will make an important advance in our knowledge of the meaning of the various parts of the ear.

So many new features are discussed, that it is impossible here to indicate more than the principal outcome of the investigation. It is now generally admitted that the ear originates in the lowest vertebrates by a modification of the superficial nervous slime-canal system, which gives rise to the conspicuous and familiar "lateral line" in fishes. Dr. Ayers, on the present occasion, elaborates this idea. It is well-demonstrated that the ear is supplied by the same nerves that are connected with the superficial sense-organs of the "lateral

line" in fishes. Not only so, but it is pointed out that the superficial slime-canal itself actually remains in distorted fragments in the perfected ear. The semi-circular canals, according to Dr. Ayers, are "simply remnants of the canal system of the surface," and "have no active part in the auditory function. They serve merely as chambers to hold the liquid necessary to float the auditory sensory hairs." The "otoliths in the ear chambers are to be considered as essentially foreign bodies . . . the result of the secretive action of the ectoderm cells, which in ancestral forms produce the surface scales. The higher we rise in the vertebrate series, and also the greater the auditory sensitiveness, the fewer the otoliths found in the ear. . . ." "The functions of the ampullar sense organs and their connected canals can hardly be different from the sense organs of the lateral line, except in refinement of function; *i.e.*, the difference can only be one of degree, and not of kind." Moreover, "in Elasmobranchs, the structural connection between the ear organs and the surface canal organs is for a long time maintained."

In conclusion, Dr. Ayers maintains that the organ of hearing in Invertebrata is totally distinct from that of the Vertebrata, and he combats the views of Retzius, who supposes that there is a fundamental difference between the ear of a lamprey and that of an ordinary fish. Dr. Beard is also much criticised.

Vanilla Disease in the Seychelles.

The large and widely-distributed family of Orchids, though much in favour with horticulturists, is one of the most useless from an economic point of view. One genus forms an exception—practically the only exception; it has given its name to the product for which it is known. The genus *Vanilla* contains about twenty species widely distributed throughout the tropics. The plants are terrestrial and herbaceous, but tall, sturdy climbers, with leathery or fleshy leaves, and bear racemes or spikes of large though usually dull-coloured flowers. The fruit or capsule, which is often elongated and fleshy, with the appearance of a pod, is very generally used for flavouring chocolate, liqueurs, &c. The best product is obtained from a Mexican species, *Vanilla planifolia*, which is extensively cultivated in other parts of the tropics.

In recent years, the cultivation of *Vanilla* has been taken up in the Seychelles, and a considerable industry established. Unfortunately, however, this has been threatened by the appearance of a disease which, in some areas, has led to much loss, as it affects the pods, causing them to turn black, and fall before ripening. In 1887, material sent from the islands to Kew was examined by Mr. George Massee, but owing to bad packing proved useless for investigation. In February, 1890, a second consignment was received representing every stage of the disease, and conclusively showing that it is caused

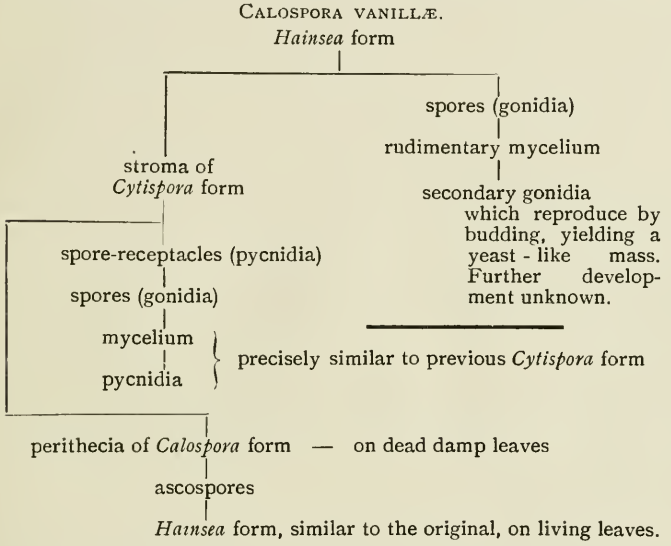
by a microscopic fungus which Mr. Massee calls *Calospora vanillæ*. In the *Kew Bulletin* for May and June last he gives an account of his examination and the results obtained. The latter are also interesting from a purely scientific point of view in elucidating the life-history of a fungus, only one stage of which was previously known. The study of the life-history also suggests a remedy as far as this can be possible without a thorough investigation of all the circumstances conducted on the spot.

Living leaves attacked by the disease show minute amber-coloured pustules springing in small groups from slightly discoloured patches, which are most abundant on the upper leaf-surface. These pustules consist of masses of spores or *gonidia*, evidently belonging to the genus *Hainsea*, borne on a mycelium which is rapidly destroying the leaf-tissue; the fungus proves to be identical with the *Glœosporium vanillæ* received in 1886 on specimens of the same species of Vanilla from Antigua, and described by Messrs. Cooke and Massee, who did not recognise the connection between the several forms, though all were present. These gonidia germinated when sown in a nutrient solution producing a mycelium and secondary gonidia; the latter when sown reproduced enormously by budding, yielding a mass of yeast-like cells, but were never seen to emit a germ-tube. No result was obtained when living Vanilla leaves were infected with them, and it is unknown to what they eventually give rise. When the *Hainsea* fungus is fully developed the leaf has become yellow as the result of its ravages, and is evidently dying. At this stage very minute clusters of black points can be seen beneath the cuticle. These on examination were recognised as black-walled spore-receptacles or *pycnidia* belonging to the form-genus *Cytispora*.

The pycnidia were partly sunk in a common cushion or stroma formed from a mycelium quite indistinguishable from that of the *Hainsea*, which preceded it. Their gonidia ooze out through the ruptured cuticle like "mucilaginous tendrils" forming pale yellow waxy masses on the leaf-surface. When allowed to germinate on the moist epidermis of an uninjured Vanilla leaf they did not enter the leaf, the germ-tube being apparently unable either to pierce the epidermis or get in by the stomata; but when placed on a *dying* leaf after commencement of disintegration they penetrated and spread through it, producing pycnidia in abundance. This shows that the *Cytispora* is a saprophyte and will not reproduce itself on the uninjured living leaf. By the time the *Cytispora* has completed its development the leaf has become dry and shrivelled, the whole of the sap and protoplasm having disappeared. No further development occurs as long as it remains dry, but if kept damp the stromata produce *perithecia* from the central portion. Though their development was carefully watched, no trace of an ascogonium or "Woronin's hypha" was seen indicative of a sexual process. The perithecia contain *asci*, each with eight *ascospores*, and exactly resemble those

of the genus *Calospora*. When placed on the uninjured upper surface of the leaves the ascospores germinate, the germ-tube enters the leaf through a stoma, and the mycelium develops in the tissues, producing a *Hainsea* form with amber-coloured gonidia pustules, in every way resembling the original.

We may represent the life-history in tabular form thus:—


CALOSPORA VANILLÆ.
Hainsea form
spores (gonidia)
rudimentary mycelium
secondary gonidia which reproduce by budding, yielding a yeast-like mass. Further development unknown.
stroma of Cytispora form
spore-receptacles (pycnidia)
spores (gonidia)
mycelium
pycnidia
precisely similar to previous Cytispora form
perithecia of Calospora form — on dead damp leaves
ascospores
Hainsea form, similar to the original, on living leaves.

The death of the plant is caused by the earliest or *Hainsea* form of this pleomorphic fungus; the two other forms appear only on the dead or dying leaf. Destruction of the leaf necessarily affects the whole plant, and symptoms of the disease may appear in the blackening tips of the "pods" before the leaf itself is visibly affected, the disorganisation of its internal tissues through which the mycelium is spreading preventing the proper performance of its functions and seriously affecting the food-supply to all parts, and especially the rapidly-developing fruits. A possible remedy, Mr. Massee suggests, may be found in the fact, brought out in this examination, that the *Hainsea* form is apparently unable to reproduce itself directly on other living leaves, but is only developed from *Calospora* spores which are themselves produced *only on damp dead leaves*. Hence it follows that the continuance of the disease depends on the presence of diseased dead leaves, and "if all fading and dead leaves are destroyed by burning, the fungus pest would soon disappear, or at least be so reduced as to produce no appreciable damage." It is probable that impaired vitality of the plants would favour the spread of the disease, as weakly specimens would almost certainly be more susceptible to attack. This idea is supported by the fact that the disease was most prevalent in flat damp places with insufficient drainage.

It is interesting to read that since the investigation just described, all the stages of the disease have been followed on cultivated orchids of the genera *Oncidium* and *Dendrobium*.

THE habits of the Sand Wasp (*Ammophila subulosa*) form the subject of a paper by Dr. P. Marchal in the *Archives de Zoologie Expèrimentale et Gènèrale* (vol. x., p. 23). The female, as has long been known, captures caterpillars which, after paralysing with her sting, she carries to the nest to furnish food for the grubs. The details have been carefully observed by Dr. Marchal, who states that the wasp inserts her sting at intervals along the median ventral region of the caterpillar, so as to paralyse the ganglia. He proved, by subcutaneous injection, that *Ammophila* does not bite her victim with her mandibles, as *Cerceris* and *Philanthus* do to the bees and other insects on which they prey, feeding themselves on the juices of the body and the contents of the alimentary canal. However, *Ammophila* was seen to bathe her tongue in the "salad" disgorged by the caterpillar, and, therefore, her instinct cannot be regarded as entirely due to a disinterested care for her offspring. In wasps which do not so carefully pick out the nervous centre for attack, the poison of the sting is stronger than in *Ammophila*, and in others with that instinct; it appears, therefore, that the venom becomes weakened as the instinct is perfected. Dr. Marchal has no doubt that the very highly perfected method of attack practised by *Ammophila* is developed from such a comparatively clumsy mode of dispatch as is adopted by *Philanthus* and *Cerceris*.

FURTHER light has been thrown on the question of the primitive insect-larva, discussed in an article in NATURAL SCIENCE for June. Professor A. S. Packard (*Zool. Anz.*, vol. xv., p. 229) calls attention to the caterpillars of the American moths, *Lagoa* and *Chrysopyga*, which, alone among the Lepidoptera, have rudimentary pro-legs on the second and seventh abdominal segments. The homology of these with the normal pro-legs seems indisputable, and, according to Professor Packard, shows that this larva "is a survivor of an ancient and very generalised type, and represents, as no other known caterpillar, the polypodous ancestor of all Lepidoptera."

A REMARKABLE habit of certain grasshoppers is recorded by Dr. F. Werner (*Zool. Anz.*, vol. xv., p. 58). Some when captured, others if kept for any length of time in captivity, bit off and sometimes devoured portions of their legs. The insects observed were of the genera *Ephippigera*, *Barbitistes*, *Saga*, and *Locusta*.

IN the last number of the *Annals and Magazine Nat. Hist.* (ser. 6, vol. x., pp. 121–128, pl. xii., 1892), Mr. Edgar A. Smith has an interesting paper on the shells of Victoria Nyanza. These mollusca closely resemble those of the basin of the Nile, and no such remarkable forms as occur in Lake Tanganyika have yet been met with. Victoria Nyanza does not possess a specialised molluscan fauna like

that lake; and of the 28 species enumerated by Mr. Smith, 12 have been recorded from the Nile or one or other of the great lakes, while of the remaining 16 so-called species very close representatives occur in other lakes and rivers of Central Africa.

Mr. R. L. Garner, whose popular work on *The Speech of Monkeys* has just appeared, left England last month for Western Africa, where he hopes to preserve by phonograph the "speech" of the apes in their native haunts. As most readers are doubtless aware, Mr. Garner proposes to establish himself in a steel cage in the forest and attempt not merely to record the vocal sounds but also to photograph any of the quadrumana he may be able to attract to his place of observation. A large audience assembled in the Anthropological Section of the British Association at Edinburgh to hear Mr. Garner's promised account of his researches; but through some misunderstanding with the officers—not altogether creditable, according to our information—the Association's distinguished guest was unable to appear.

In a lecture delivered recently before the Geologists' Association, Mr. Rudler gave an interesting account of the pioneers of British Geology. He pointed out, as was done years ago by Buckland and Conybeare, that to a Welshman is due the credit of first showing that strata succeed one another in a regular manner through large tracts of country. This "Patriarch" of Geology, George Owen of Henllys, Lord of Kemes, was born in 1552 and died in 1613; he left a valuable manuscript work in which he "traced with much accuracy the direction and extent of the strata of coal and the limestone which accompanies them through the whole of South Wales, and pointed out the connections of this tract with similar districts in Gloucestershire and Somersetshire." Owen's original work, "The Description of Penbrokeshire" (so spelt), has lately been published by the Society of Cymmrodorion.

About two years ago a "Coal Search Committee" was founded with much *éclat* for the "systematic investigation of the deep-seated geology of the South-East of England." An imposing list of names was appended to the original circular, which was signed by Mr. James T. Day, and a meeting was presided over (according to the circular) by the Right Hon. the Lord Mayor of the period, supported by the "Coal Search Committee." It would be interesting to know what has been done in the matter, or what has been done with the funds which were subscribed by the over-cautious in the early days of the boom. Perhaps some of our readers can enlighten us?

In the *Annals of Botany* for July, Professor J. R. Green gives an account of his investigations on the digestive ferment of the Kachree

gourd, *Cucumis utilissimus*. Ferments capable of digesting proteid matter have already been shown to exist in various parts of plants—the insectivorous leaves of *Drosera*, the pitchers of *Nepenthes*, the germinating seeds of the Lupin, and the Castor Oil plant, the fruit of the Papau, and the latex of the Fig. The Kachree gourd has, in India, the reputation of possessing the same properties as the two last-mentioned plants, and Professor Green shows conclusively that it contains a ferment which is very similar to papäin, the vegetable trypsin of the Papau. The expressed juice of the central pulp, and a watery extract of the pulp just beneath the rind, were both found capable of dissolving coagulated egg albumen. As the disappearance of the albumen was much more rapid when a 3 per cent. salt solution was used, it is concluded that the ferment is either a globulin in nature, or, more probably, associated with a globulin constituent in the plant cells. The digestive power of the watery extract would be explained by the presence of inorganic salts in the plant, which would assist solution on addition of water. The ferment resembles papäin in working best in a slightly alkaline solution, less readily in a neutral one, and least of all in the presence of an acid. Like papäin, too, it is allied to the trypsin rather than the pepsin of the animal organism, as it effects a very complete decomposition of the albumen, giving rise to peptone and later to leucin.

According to the *Kew Bulletin* for May and June, the consumption of Paraguay tea or *Maté* in South America, rivals that of the Eastern herb in the Old World. It is extensively used by the whole population, "upwards of five million pounds" being exported from Paraguay alone. Its preparation is apparently somewhat rougher than the Chinese or Indian method; the leaves are scorched and dried on the branches brought in by the collectors, and then beaten, separated, coarsely ground by rude mills, and packed in skins and leather bags. In use, the leaves are infused in small tea-pots, and the liquid sucked up through a tube with wire network or perforations at the bottom. The active principle in the leaves is caffein, an alkaloid identical with that found in the ordinary tea and coffee. There is, moreover, a large amount of tannic acid, according to Dr. T. C. Charles, who also tells us in a note in the *British Medical Journal* of July, 1890, that *Maté* is closely allied to coffee, but is slower in yielding up its nutritious principles to boiling water. To the first-mentioned periodical Mr. N. E. Brown supplies a note on the botany of Paraguay tea, which is apparently obtained from several species of *Ilex*—a genus known to us in the common holly (*Ilex aquifolium*)—and a species of *Symplocus* and *Elæodendron*. The most important is the *Ilex paraguariensis* of St. Hilaire, of which there are several varieties, probably yielding different qualities of tea.

I.

The Foundations of Science.

IT is most reasonable that a publication devoted to "Natural Science" should accord some of its space to the consideration of the foundation upon which the whole of that science necessarily reposes. I therefore hail with pleasure Mr. Ryle's defence of Professor Pearson's "Grammar," although I regard that defence as mistaken and exaggerated, and the work it defends (which I have carefully read) a singularly unreasonable one.

Everyone knows that "Idealism" is a metaphysical system, and Mr. Ryle tells us expressly that "Professor Pearson adopts a philosophical position which may be fairly described as thoroughly idealistic." This is true. I have elsewhere said[1] that "he is an idealist of a kind." Nevertheless, if there is one thing Professor Pearson repudiates and blames again and again, it is philosophy. Any defence must be a mistaken one which speaks eulogistically of an author being that which the man himself emphatically denies that he is.

My object, however, in this short paper, is not to engage in a triangular duel, but simply to point out what is that basis upon which all "Natural Science" necessarily reposes.

Professors Ryle and Pearson, faithful to the traditions of their school, declare that we can know nothing but feelings—"sense-impressions and sense-impresses"—received, associated, remembered, &c., and so issuing in "concepts." The ultimate appeal of this school is to "the senses," and it is therefore justly spoken of as "*Sensism*."

To me it is clear that not only every conception, but also every human perception, contains what is altogether beyond sense,[2] and that our ultimate appeal is not to the senses but the intellect. This system may, therefore, rightly be termed "*Intellectualism*."

It is time that the real basis of science should be generally understood; that men should no longer be bound by the fetters of tradition and called upon to bow down "lowly and reverently" to those who are represented to them as "their betters" by the voice of "authority." It is time for them to use freely their own reason, and while unflinchingly and fearlessly recognising what they do not and cannot know,

[1] See *Nature*, July 21, 1892, p. 267.

[2] See *The Origin of Human Reason*, p. 280.

to also recognise what they do know, what they must know if they know anything, and what is that solid foundation on which alone they can securely rear any self-consistent and stable fabric of knowledge.

It would occupy too much space were I to enter upon any defence of the evident and fundamental truths of science, nor is it needful, as I have done so elsewhere both briefly[3] and also more at length.[4]

There are four essential truths concerning what we know :—

1. We can know not only our actions, sensations, imaginations, reminiscences, emotions, perceptions, and conceptions, but also our own substantial and continuous personal existence.[5]

2. We know, through memory, real existences external to all our present experience, *i.e.*, we have the faculty of knowing what, *pace* Mr. Ryle, is objective truth.[6]

3. We know that there are truths which are true not only here and now, but which must be true ever and always—as that " nothing can at the same time both be and not be."[7] We know this to be not merely a law or condition of our own mind, but that it is true of all " things-in-themselves " apart from the existence of any or every mind.

4. We know that, if certain principles be true, then whatever logically follows from them must be true likewise.[8]

Unless we know these things science is logically impossible, and any scientific man who denies them either deceives himself or seeks to deceive others. Without them it is impossible to have—what alone should satisfy a man of science—a system of knowledge complete and harmonious, satisfactory and stable, from its deepest foundation to its loftiest pinnacle.

If these truths are *really* doubted or denied by anyone, he must fall into a state of mental paralysis and intellectual inanition; though, of course, men may really hold and act on them, while supposing or pretending they do nothing of the kind,[9] there being so much both of stupidity and dishonesty in the world.

Really, however, to believe that any appeal lies to sensation from that which the intellect declares to be evidently true, is a self-stultifying belief. How, save by the intellect, can we know that we possess a sensitive faculty, or that we have " sense-impressions " and " sense-impresses " at all ?

But if we know so much, *i.e.*, if we know the four truths above enumerated—what difficulty is there in recognising that we may know and do know yet more ?

[3] See *Nature*, Nov. 19, 1891, pp. 60–62; and Nov. 26, pp. 82–85; also Supplementary Notes in the numbers for Jan. 7, 1892, p. 222, and Feb. 11, p. 343.

[4] See *On Truth, a Systematic Inquiry*, Kegan Paul, Trench & Co., 1889.

[5] See *On Truth*, pp. 3–28.

[6] *Op. cit.*, pp. 29–37.

[7] *Op. cit.*, pp. 39–46.

[8] *Op. cit.*, pp. 53–59.

[9] *Op. cit.*, pp. 134, 135.

For myself, I am absolutely convinced (after wandering for years in the mazes of Idealism) that we do know vastly more. The "sense-impressions" and "sense-impresses" arouse the intellect to much more than their own recognition, association, &c., &c.; they arouse it to the direct perception of external objects—of things-in-themselves—though not, of course, to an exhaustive knowledge of them. It is evident to me that, in Mr. Ryle's words, "they have a 'reality' not conditioned by the nature of my faculties for knowing them."

That the knowledge of the numerical relation of things—that the facts concerning their number (*e.g.*, that oranges on a plate are three and not five) are real and absolute, and not conditioned by my faculties, is, for me, unquestionable. I am also convinced that I have an intuition of the extension of extended things. Dr. Johnson's often-ridiculed kick at a stone by way of refuting Idealism, was most probably a mute affirmation of his consciousness of this intuition, and his recognition that a synthesis of "sensations" and "sense-impresses" is quite inadequate to account for our perception of an external body or of our own body.[10]

But though I am convinced I know the before-enumerated four truths, and the existence, figure, and numerical relations of extended things, I do not know how I know such things. This, however, is an ignorance of small consequence, and an analogous ignorance is absolutely inevitable, whatever theory of knowledge or philosophical system we may adopt.

Messrs. Ryle and Pearson, and their masters and pastors—*i.e.*, the "sensists"—are in the very same difficulty with the Intellectualists. How it comes about that their organisation is endowed with the power of having "sense-impressions" and "sense-impresses"—how the stimulation of a nerve and brain can result in a feeling—it is absolutely impossible for them to say. They do not, however, on this account doubt their possession of them, or the truth of their persuasion to that effect. Neither does the inability of the Intellectualist to say how it comes about that on the occurrence of the necessary conditions he perceives a material body, a thought, or an abstract necessary truth, in the least cause him to doubt a thing which is to him evidently true. Our feelings are but the *means*, not the *object* of perception.[11]

However great our knowledge, we must, unless we have recourse to a *regressus ad infinitum*, always arrive at ultimate cognitions—that is, at facts we cannot explain; and it is no greater difficulty or absurdity to recognise that we have "intellectual perceptions" which are ultimate and inexplicable, than that we have "feelings" which are ultimate and inexplicable; moreover, there cannot possibly be any further test of truths so self-evident.[12] Ultimate truths must contain their own evidence.

[10] As to this consult *On Truth*, pp. 90–96.

[11] *Op. cit.*, pp. 90 and 91.

[12] *Op. cit.*, pp. 10–14.

Mr. Ryle, in speaking of Space and Time as represented in the Grammar, says that what Mr. Pearson "has disputed is the conventional, or uncritical view, that takes space and time for 'things,' instead of taking them for an order of things." But, in fact, they are neither the one nor the other. One "order of things" is Extension, that is, many things are extended, and extension is an abstract idea and term denoting the extended nature of such things, which nature is a reality and real condition of the things referred to. Space, however, is not extension, but is altogether ideal, an abstraction from abstractions, and is nothing but a conception of the extension of all extended things. It is the abstract idea of their common extension and mutual exclusion.[13] Similarly, succession is another "order of things," *i.e.*, many things succeed one after the other. Succession is an abstract idea and term denoting the succession of things which succeed. It is, therefore, real as a quality of succeeding things. But Time is not succession—it is altogether ideal, and again (like Time) is an abstraction from abstractions, and nothing but a conception of the succession of all succeeding things.[14]

This comprehension of the meaning of the terms time and space, accompanied with a recognition that we can intue succession as well as extension, and that the four truths earlier enumerated are true, furnishes us with a secure basis upon which the whole series of sciences, from Mathematics to Philosophy, can progress in harmonious self-consistency.

But there is another truth, the clear perception of which has a special relation to that department of "Natural Science" which we distinguish as Biology. For we know most intimately by and in our own consciousness, something—our own intellect—which exists continuously, which is conscious of successive objects and events, and which, itself transcending them, can recognise them as forming a series which it can contemplate as a whole or in parts, and in different orders, according as may be desired. This power or principle it also knows with perfect certainty can and does know itself, is aware of the kinds and directions of its activities, and can regard them as a whole, or in groups, or singly. It can, it well knows, perceive its own states, both passive and active, and also external objects and events, and can compare the relations between them, returning upon itself at will along different lines of thought, and also setting forth in various directions at will. Such a power, aware of all these things, and present to them all, cannot itself be multitudinous, but must be as much a unity as anything we can think of. It is also evident and certain, therefore, that this principle is neither a material substance nor a physical force, but presents the greatest contrast to both, and the greatest contrast to what is material is what is immaterial. Also, since each man knows that it

[13] *Op. cit.*, pp. 407–409. [14] *Op. cit.*, pp. 409–410.

is he who not only thinks but also feels, he knows, if he reflects upon it, that his body and this thinking principle are, to his experience, one unity.

Therefore, each of us is a unity with two sets of faculties, material and mechanical on the one hand, immaterial and non-mechanical on the other. No certainty we can attain to about any other object can be nearly so certain as this certainty we have about our own being, above all our immaterial being;[15] first its dynamic, immaterial aspect, and secondly its material, mechanical aspect. That each man is a material, definitely organised substance in one unity, with a dynamic, immaterial principle of individuation revealed in consciousness, is the first truth of physical science. It is emphatically the fundamental truth of Biology; for biology deals with living things, and of no living thing can we have so complete a knowledge as of ourself, because it is only to the investigation of our own being and activities that the most direct kind of knowledge—our conscious perceptions—can be directly and immediately applied.

This truth, which a careful examination of our own being reveals to us, has very wide and important results when the phenomena of life, especially animal life, are studied in relation to it. By it, and by it alone, can we obtain any intelligent apprehension of a multitude of physiological phenomena. On this question, however, I do not here propose to dwell, I have only referred to it as a further —as a culminating—instance of the effect which the study of our own intellectual activity must have on our comprehension of the world around us. In the historical order of knowledge—the knowledge of the individual as well as of the race—the study of what is external to us does come, and must come, first. But as we emerge from, and put away, the things of childhood, we have more and more to reverse this process. For any satisfactory comprehension of natural phenomena, and especially for those of living nature, the first and most pressing need now, is an adequate acquaintance with our own higher mental processes. Such investigation is much less easy than is the study of what is external to us, but it is more important because most fruitful. The certain, though gradual, effect of its faithful prosecution will most certainly be the overthrow of "*sensism*," and its replacement by the only system which is satisfactory in its application on all sides, the system which distinctly recognises that the basis of all science must consist of truths recognised by thought as self-evident and necessary—the system of "*Intellectualism*."

St. George Mivart.

[15] *Op. cit.*, pp. 390–391.

II.

Biological Theories.

I.—THE NATURE OF HEREDITY.

THE numerous extant theories of heredity all begin with the assumption that some characters are transmitted from parent to offspring, while they differ as to the "transmissibility" of certain others. In the case of characters which they all assume to be transmitted, they differ largely as to the mechanism of the transmission. Haeckel holds, or at any rate did hold, that the medium of transmission, the carrier of the influence which produced new characters like the old ones, was a kind of molecular movement. Darwin imagined certain particles derived from all parts of the body to serve the same function. Both agreed as to the transmission of some "acquired" characters. Weismann opposes both these views and relies upon a supposed continuity of germ-plasm for the explanation of the transmission of all except "acquired" characters, and of these last he denies the transmissibility. The other theories which are held by modern biologists are all more or less similar to one or other of these two last—some rely upon continuity of something or other, the rest upon the transmission of material particles derived from various parts of parental and ancestral bodies.

I do not propose to weigh afresh the relative claims of these two schools of theorists, nor to ally myself with either school by producing a new theory in conformity with its fundamental principles. Both schools appear to me to be equally wrong, and to have set out with equally false notions as to the nature of the phenomenon they have attempted to explain.

The phenomenon I propose to explain is the relative constancy of specific form in several generations of any species of animal or plant, *i.e.*, the reappearance of certain characters in many individuals belonging to several generations. It is not a transmission of characters from parent to offspring alone, not only the reproduction in the offspring of characters possessed by the parents or more remote ancestors; and this will be shown by the first example I shall bring forward in illustration of my view.

In each successive generation of termites or "white ants," are found certain individuals, called "soldiers," which are blind and wingless, and which are incapable of any reproductive function. They have enormous heads, much larger than any of the other

individuals have, and their jaws are more powerful. The soldiers of one generation are indistinguishable from those of any other; their specific characters are constant. This constancy of specific characters, this reproduction, generation after generation, of individuals indistinguishable from one another, this likeness among individuals of different generations, is a phenomenon belonging to the class I have referred to. Whether it is "heredity" or not all depends upon the definition of the term. If "heredity" means the transmission to the offspring of characters possessed by the parent, then this is not heredity, for the soldier termite is very unlike its parents, and also unlike all its remoter ancestors. All its ancestors, so far as we know, for thousands of generations, have possessed wings and eyes, and the power of reproduction, while none of them have possessed the enormous head and powerful jaws which form a most striking character of the soldier. The soldier is exactly like millions of other individuals from which it is not descended, and which cannot, therefore, have transmitted their characters to it; it is unlike all those which might have been supposed to transmit these characters, but for the fact that they have never possessed them.

This example is not given to disprove any other theory, and I do not think that it would be altogether impossible to explain it on the assumption of pangenesis or of continuous germ-plasm, as satisfactorily as those assumptions explain more commonplace forms of heredity. I give the example merely to aid in the exposition of my own view.

If we examine into the factors which by their co-operation have determined the form and structure and consequent functions of the soldier termite and of its parts, we may recognise among them several classes. Whether the soldier develops from a fertilised egg or not I do not know; we must, therefore, to be on the safe side, assume that it at least may do so. The factors may then be classed as follows:—

1. Those determining the constitution of the ovum at the time of fertilisation.
2. Those determining the constitution of the spermatozoon at the time of fertilisation.
3. Those acting upon the fertilised ovum and the developing soldier from without.

Among the factors determining the constitution of the ovum are to be included the constitution of the female parent. Its structure and consequent functionality—for I include chemical as well as physical constitution in "structure"—have been determined by factors which might be classed in the same way as we have classed those determining the structure of the soldier. The origin of these factors I shall have to discuss in a future paper; for the present, we may take the structure of the parent as our starting point, and similarly also with the male parent. One result of that structure and consequent functionality of the mother is the production of ova. Though all were formed in one ovary, it would not follow that all should be alike,

any more than it follows that all cells of the body are alike because all have arisen from one egg. There may be many kinds of ova produced, and the development of these into insects of at least six different kinds may possibly be due to some initial difference in the ova. The same is true of spermatozoa (*mutatis mutandis*).

Again, the ova may be fertilised at different stages of maturation. There is a difference between a mature and an immature ovum, and there are stages in maturity. The ovum is a living cell, and its constitution, chemical and physical, may vary with its age. The age at which it is fertilised may, therefore, have something to do with its future development. So also with the spermatozoon, and some have held that differences of this kind determine the sex of the offspring in various animals, and especially in mammals.

If, however, two females are exactly alike in every detail—physiologically as well as structurally—then, however many kinds of ova A produces, B will of necessity produce kinds of ova exactly like them: and a similar likeness will also exist between the spermatozoa of two males, M and N, however many kinds there may be of them. If two like ova, derived from A and B respectively, be fertilised by two like spermatozoa, derived from M and N respectively, then the two fertilised eggs so produced will be alike in every minutest detail, physically and chemically, and, therefore, also physiologically.

Of the influences brought to bear upon the fertilised and developing egg, the most important would seem to be temperature, supply of oxygen and of moisture, and quantity and quality of food. We know that the quantity and quality of food supplied to the larva developing from the fertilised egg of a hive bee determine whether that larva shall become a queen or a worker. We do not know this of the termite larva, but it is none the less safe to say that if every influence brought to bear upon each of the two like eggs just spoken of be precisely the same in the two cases then those two like eggs will undergo like changes. Whatever the form and structure of the full-grown insect developing from one of those eggs, that developing from the other must of necessity be like it. The causes are *ex hypothesi* exactly alike, the effects must of necessity be alike also. If one of these two insects be a soldier termite, so must the other be. If one be a butterfly, so must the other be.

The likeness of the influences determining not only the production, but also the *formation*, the determination of every detail of structure, in any two cases, sufficiently explains the likeness of the two individuals. And this is true even when the two individuals are only distantly related to each other, and when neither of them bears any close resemblance either to its parents, or to any of its ancestors.

A particular soldier has no eyes and no wings, because the influences which determined the structure of that particular soldier were not such as to lead to the production of either wings or eyes. A second soldier is also without eyes and

without wings, and for the same reason. Not a single ancestor of either of them was, so far as we know, devoid of either wings or eyes. Winglessness and blindness are constant characters among soldier termites, not only in one species, but in several; not only on one continent, but on several; and there is reason, therefore, to believe that winglessness and blindness were already constant characters, even before the existing species were differentiated from one another—were even constant characters before any existing species was evolved from the common ancestral type from which they have descended.

In the lungs of a common frog are frequently found several parasitic worms belonging to the species *Rhabdonema nigro-venosa*. Each individual *Rhabdonema* is male when young, and later in life becomes female—in other words, it is a protandrous hermaphrodite. The old ones (that is the females) produce ova, which, after being fertilised by the spermatozoa of the younger ones (that is of the males), are carried from the lung to the mouth of the frog, and are then swallowed and pass with the food through its intestine, developing, as they go, into *Rhabdites*. These are ejected with the excrement of the frog, and develop, in moist earth, into sexually-mature worms (*Rhabdites*), of which some are males and others females. They are never hermaphrodite, and they are, in other respects, unlike their parents. The few ova produced by a female are fertilised by the spermatozoa of a male, and, after feeding for a time upon the tissues of the mother's body, escape, and find their way into the lungs of frogs, there developing into *Rhabdonemata*, like their grandparents and unlike their parents, and passing through a condition in which all are male to a later condition in which they are female.

I fail utterly to see how this resemblance of individuals to their grandparents, and unlikeness to their parents, this regular alternation of generations of hermaphrodite *Rhabdonemata* with generations of male and female *Rhabdites*, is to be "explained" by the continuity of germ-plasm, and I see no necessity for assuming the transmission of two sets of pangenetic gemmules which take it in turns to determine the structure of the successive generations. It is very easy to believe that the influences at work producing ova and spermatozoa in one pair of *Rhabdites* are very like those doing similar work in another pair, and seeing that this likeness in itself will explain the likeness of the *Rhabdonemata* which develop from these ova and spermatozoa in one case, to the *Rhabdonemata* produced in the other case, there is no need for any assumption beyond that. So long as causes are alike the effects are alike, and the assumption of the existence of pangenetic gemmules, or of an unproven continuity of a special germ-plasm appears to be uncalled for.

I am almost afraid to refer to the fact that external influences have some share in determining the form, not only of the developing individual, but also of its offspring, because, whenever I have done

so in the past undue importance has been attached to the part I have supposed them to play. To guard against this misunderstanding as far as possible, I will refer to a case in which it must be obvious that I do not suppose that the particular influence I shall refer to has anything to do with heredity in any accepted sense of the word. I regard the likeness of one individual to another as arising from a likeness in the causes which determined the characters of the two individuals respectively. Into those causes some external influences have entered as factors, direct or indirect, but the external influences dwindle into insignificance when compared with the internal ones.

Professor Weismann records an experiment of his, in which he cut off the tails of many very young mice of half-a-dozen successive generations. All those mice, when fully developed, were tailless mice. They resembled each other in being mice by virtue of the action of internal influences, which were approximately alike in all cases. They resembled each other in being tailless by virtue of the action of one particular external influence, which was alike in all cases. So long as that external influence remained constant, taillessness of the full-grown mice was a constant character, *and no longer.* When the influence to which that taillessness was due was removed, the taillessness ceased to recur.

If some change in the conditions of existence should so alter the constitution of mice that the fertilised eggs they produce should have such structure as would lead to their development into tailless mice, then there is reason in believing that the maintenance of the changed conditions would convert the mouse into a tailless species, and that the taillessness of the species would remain a constant character so long as no further change in the conditions of existence occurred, *and no longer.* Though the specific characters are determined by the joint operation of internal and external influences, the internal influences are infinitely more effective than the external, as they are also infinitely more complex. But, on the other hand, the internal influences depend both directly and indirectly upon the external ones. And, further, none but slight changes in the external influences are compatible with the continued existence of the species.

The direct, though perhaps remote, influence of external influences upon the internal constitution, and hence more remotely upon the constitution of the ova and spermatozoa, and hence again more remotely still upon the form and structure of succeeding generations, is well seen in the transformation of a stock of *Artemia salina* into *A. muelhauseni*, and *vice versâ* by Schmankewitsch through alteration of the strength of the salt-water in which the animals were living. This result is so interesting that it is surprising that nobody with the means at command has yet been found to verify it by further experiments. The transformation of a stock of *Artemia* into *Branchipus* is so astounding, that though it might strengthen my argument I dare not assume it to be true till it has been verified.

Semper's experiments upon *Limnea*, though similar, are less striking examples of the direct effect of external conditions upon the development of each individual. They show the size of a Limnea to be as much dependent upon external conditions as was the taillessness of Weismann's mice.

The life-history of Phylloxera or of Aphis is an excellent example of the direct effect of the environment, including food-supply and temperature, upon the form of the offspring. That is, upon the form and structure of the generation as yet unborn, through its effect upon the internal influences determining the structure of the germs in process of formation. In Phylloxera, four successive generations, all different from one another, intervene between each two generations, capable of amphigonous reproduction. More generations may intervene—that depends upon external conditions—but these four, all different from one another, are tolerably constant, and the individuals of generation A in one year are like the individuals of generation A in any other year, though unlike the ancestors of any of the four next preceding generations. To the question "Why?" my answer is the same as in the case of the *Rhabdonemata*.

One more example will suffice. John Jones resembles his father so closely that it is impossible to say which of two photographs taken at similar ages is the portrait of John and which is that of his father. This I hold to be due, not to the continuity of the spermatic portion of the germ-plasm of two successive generations, but to the reproduction, in two generations, of a complex set of influences which, by their interaction and co-operation, have determined the form of the father in one generation and of the son in the next, and these influences I take to be both paternal and maternal, both internal and, to a very slight degree, external also.

All the cases which I have given as illustrations have one thing in common. In every case I have referred to the occurrence of some individuals like some other individuals belonging to an antecedent generation, though the individuals resembled are in some cases parents, in others grandparents or ancestors more remote, and in the case of the termite soldier the individuals resembled are not ancestors at all. In every case I hold the likeness between the two individuals to be due to the likeness of the causes which have determined the formation of those individuals. B is like A because its form and structure have been determined by influences like those which have determined the form and structure of A. When B is the direct descendant of A, it is customary to call the phenomenon "heredity." Rightly or wrongly, I hold the phenomenon to be of the same nature in all cases, and to be simply an illustration of the general law: "Like causes produce like effects."

How the like causes are produced, I shall endeavour to explain in a future paper, as also the phenomena of atavism and variation.

C. Herbert Hurst.

III.

The Evolution of Oceans and Continents.

NATURAL Science is indebted to Dr. Wallace for clearing away some misconceptions which have surrounded the question of the permanence of Oceans, and for imparting some freshness to the controversy by adducing several new arguments in its favour. Hitherto the question has been discussed rather as if it were necessary to hold one of two extreme views; the one view being that the great continental plateaux and oceanic depressions were formed at a very early period of the earth's history, and have occupied their present positions ever since; the other view being that the movements of the earth's crust have been so frequent and so great that no part of it has remained either in the state of sea or land through many periods of geological time.

I will not stop to enquire whether Lyell believed in the possibility of such frequent interchange when he wrote that "continents, although permanent for whole geological epochs, shift their positions entirely in the course of ages." The extreme view may have been held at one time when little was known regarding the depths of the ocean, but few geologists of any repute would maintain it at the present time. On the other hand, the idea of the fixity of the oceanic and continental areas is supposed to have received strong confirmation from the investigations of the "Challenger" Expedition. It was argued that modern oceanic deposits differ so much from any rocks which form part of the continental plateaux that none of these rocks could be of oceanic origin, and consequently that oceans and continents could never have changed places. It was confidently stated that *all* ancient marine formations had been formed in comparatively shallow water, and it was even predicted "that representatives of the abysmal deposits of the central oceans are not likely to be met with among the geological formations of past times." [1]

The supposed absence of oceanic deposits from continental areas was regarded as proving that the interchange of sea and land was confined to areas within 200 or 300 miles of the continental plateaux where the water was less than 1,000 fathoms in depth. In this way 92 per cent. of the whole oceanic area was excluded from the regions of possible interchange. Against this view I have protested as being

[1] Sir A. Geikie. "Text Book of Geology," 1st edition, 1882, p. 608.

merely the other extreme of the swinging pendulum of scientific imagination.

Recent discoveries of oceanic deposits, such as must apparently have been formed in depths of from 1,500 to 2,000 fathoms, and including representatives both of "red clay" and radiolarian ooze, but now raised high above the level of the sea within the limits of a continental plateau, have shown the danger of depending on negative evidence. Again, a more accurate knowledge of submarine contours and the exigencies of explaining the distribution of animals have obliged Dr. Wallace to include much greater depths within the regions of possible interchange. He now says: "All that is necessary to maintain therefore is, that existing continents with their included seas and their surrounding oceanic waters as far as the 1,500-fathom, or in some extreme cases the 2,000-fathom line, mark out the areas within which the continental lands of the globe have been built up; while the oceanic areas beyond the 2,000-fathom line, constituting, according to Mr. Murray's data, 71 per cent. of the whole ocean, have almost certainly been ocean throughout all known geological time."

This is a great concession, and the "almost certainly" may foreshadow further admissions; 71 per cent. is very different from 92 per cent., and the extension of interchangeable areas to regions within the 2,000-fathom line enables us at once to speculate on the possibility of a former antarctic continent with such extensions toward New Zealand, South Africa, and South America as will explain the curious points of similarity in the modern faunas of these countries. The existence of such a continent has already been suggested by Dr. W. T. Blanford.[2] Dr. Wallace's present view also enables us to assume the former existence of a large continent in the Western Pacific with extensions southward to New Zealand and westward to Australasia. It also gives us great possibilities of change in the Atlantic Ocean, though hardly sufficient to account for all the palæontological evidence which has been or can be adduced in favour of old connections between the more tropical parts of Africa and America.

So far, therefore, Dr. Wallace has departed from the extreme views which have been held by the writers whom he quotes with approval, and which he upheld even in the second edition of his "Island Life," published only a few months ago. He therein argues against the deep-sea origin of chalk, and repeats the very erroneous statement that "deposits uniform in character and more than 150 or 200 miles wide were rarely, if ever, formed at the same time." Why does he vainly endeavour to deny the close similarity between chalk and foraminiferal ooze when in a later chapter he contemplates submergences of more than 1,000 fathoms in extent?

[2] Pres. Address to Geol. Soc., 1890, p. 104.

He now advances certain arguments against the wholesale interchange of oceanic and continental areas, but the force of his reasoning is greatly weakened by a misconception which appears to underlie the whole of it. He seems to think that those who oppose the theory of the permanence of oceans must believe in the possibility of a brand new continent rising whole and complete from the ocean depths while an older continent was compelled to vanish beneath the waters in order to preserve the terrestrial balance. I am not aware that anyone has ever propounded such an audacious hypothesis. I have never seen it seriously suggested that an entire continent has been contemporaneously submerged, nor has any modern geologist contemplated "the building up of a continent the size of Africa from the mean depth of the ocean." Certainly Dr. Wallace is not warranted in imputing such crude ideas either to Dr. Blanford or to myself, and every geologist will admit that "the remarkable parallelism and completeness of the series of geological formations in all the best known continents indicates that none of them have risen from the ocean floor during any portion of known geological history."

Those who oppose the doctrine of permanence say that the present continents are the outcome of a long series of geographical mutations, and I would add that each phase was an episode in a long process of geographical evolution. There is good reason to believe that even in Pliocene time the outlines of the continents were very different from the present, some areas now below the sea being then above it, while other tracts then beneath oceanic waters have since been raised into dry land. We know that Miocene geography differed still more greatly from that of to-day, and it is not therefore unreasonable to suppose that in the Cretaceous period large parts of the modern oceans were land, and large parts of the modern continents were portions of the ocean, the continental connections being totally different from what they are now. In short, the interchange we believe in is the frequent interchange of small portions of oceans and continents, till, in the course of time, the accumulated changes have accomplished great geographical mutations.

Against this view of interchange it does not seem to me that either the first or the third of the considerations mentioned by Dr. Wallace are very powerful objections. With respect to the second argument, that there are no irregularities on the ocean floor corresponding to those on land, I would point out (1) that it applies most completely to the deeper and consequently to the oldest portions of the ocean floor, (2) that we really as yet know very little about the details of sub-oceanic contours, (3) that long-continued deposition must tend to obliterate any pre-existent irregularities, (4) that such inequalities are likely to have been much smaller than those shown in continental areas, because the continents are probably those

portions of the earth's crust which have been subjected to the greatest pressures and disturbances; while the deeper parts of the ocean are probably those where the crust has long been in a state of tension and comparative quiescence. It is true that the tension might, and probably has, led to the production of faults; but the faults developed at any one epoch would be small, and if they formed a system of trough-faults the surface features produced would be quite insignificant. (5) Lastly, if there be any truth in Mr. Fisher's idea of the frequency of sub-oceanic outpourings of lava, such extravasations would largely account for the comparative smoothness of the ocean floor.

Here, so far as Dr. Wallace's arguments are concerned, I might leave the subject, but I wish to make a further suggestion, and, following Dr. Wallace's example, to advance another *à priori* consideration.

All who have hitherto discussed the question of the permanence of oceans have done so on the basis of the present physical conditions of the globe, and have assumed that the volume of ocean water, and consequently the mean depth of the ocean, have always been the same. But are we justified in making this assumption?

The Rev. Osmond Fisher has done a great service to the science of Geology in showing that the contraction theory is totally inadequate to account for the inequalities of the earth's surface, and in placing the hypothesis of a thin crust with a liquid substratum upon a sound basis by his suggestion that water in the state of gaseous matter is associated with the liquid magma. In this way he explains the presence of water among volcanic products, which has always been a physical difficulty. But, if his theory be true, it follows that every eruption which has taken place on the globe during the long ages of past time has contributed a certain amount of water to swell the volume of the ocean. Hence the oceans have been gradually increasing in depth and extent throughout all geological time, and the area of the land has been as gradually diminished.

If, therefore, we accept this hypothesis (and it affords by far the most satisfactory basis for the explanation of terrestrial physics that has yet been imagined), we must look back to a time when the oceans were very much smaller and shallower than they are now. Further, if we accept Mr. Fisher's view of the manner in which continents and mountain chains have been ridged up by the pressure of the horizontal connection-currents in the substratum, it seems probable that there has been a tendency toward the condensation of land masses into continental areas, and we may look back to a time when the continental plateaux did not rise so high above the mean level of the ocean floor as they do now.

It would appear, then, that a geologist is justified in supposing the total area of land in early Palæozoic time to have been equal to or greater than that of the sea, both being more equally distributed,

so that there were neither great oceans nor isolated continents in our modern sense of the terms. From this condition of geographical equality, it seems to me there has been a gradual tendency to the development of high continental plateaux, while the deeper sea-troughs have been merged into oceans, and the volume of the surface waters has been continually increased by the extrusion of the water-substance from the interior of the earth.

On this hypothesis we can at once explain the rarity of extensive oceanic deposits of the modern type among the great series of stratified rocks, for in the comparatively narrow and shallow seas of Palæozoic time no such deposits could be formed. Now, as a matter of fact, while deep-water muds are common among Palæozoic rocks, no rock which is likely to have been analogous to a modern abyssal ooze has yet been found, for the radiolarian chert of Ayrshire can hardly be claimed as such, either in character, thickness, or extent. Coming down to Mesozoic time, we find the Cretaceous Chalk, which does in all three respects bear great resemblance to a modern calcareous ooze, but it is not associated with abyssal deposits of the red clay and radiolarian ooze types. When, however, we reach Tertiary times the West Indian deposits furnish proof that the oceans were deep enough for the formation of all the modern types of oceanic deposits.

On this hypothesis of the gradual evolution of oceans and continents we can imagine the former existence of continental land where the ocean now rolls, without depressing any modern continent below the ocean to produce it. In short, we may believe that the places of our continents have *nearly* always been occupied by land or by shallow seas without assuming that the land areas have always been restricted to the present continental plateaux. We may likewise believe in the great antiquity of the deeper parts of the oceans, without assuming that these oceans have always been either as deep or as extensive as they now are.

In his Appendix (p. 9), Mr. Fisher thinks it probable that his conclusions as to the structure of the sub-oceanic crust do not apply to the borders of the oceans, and that the one type of crust changes gradually into the other type, "so that there is an intermediate belt of which we cannot assert that it belongs exclusively either to the oceanic or to the continental type." This allows us a certain amount of latitude; but I do not think he goes far enough, and believe his own conclusions must eventually lead him into the very theory of evolution which I have just suggested. For instance, he accounts for the greater density of the upper layer of the typical sub-oceanic crust by supposing it to consist of extravasated basic lavas; but the extravasation of these lavas must have gone on *pari passu* with the compression of the continents, and hence the very construction of a special type of sub-oceanic crust must have been a process of gradual development. In dealing with geological time, Evolution, as distinct from

Restorative or Catastrophic Change, must be our principle of interpretation.

Here, however, it may be objected that Mr. Fisher has supported the theory of the permanence of oceans, and that he assigns to the sub-oceanic crust a structure which differs from that of the continental crust. In his Appendix, however, he has partially altered his conception of the structure of the sub-oceanic crust, and he has recently read a paper before the Cambridge Philosophical Society in which he arrives at results which tell strongly against his speculation about the origin of ocean basins; in this I much rejoice, for that chapter in his book had never commended itself to me.

It must be remembered, too, that Mr. Fisher has not attempted to enter into the history of the sub-oceanic crust. His position is simply this; his calculations show that, if his hypothesis be true, there are large tracts of crust which must have the special structure he ascribes to them; he does not assert that they have always had that structure, but he does see a difficulty in supposing that, having acquired that structure, they can ever again become continental land.

A. J. Jukes-Browne.

IV.

Recent Advances in Knowledge of the Ichthyosaurian Reptiles.

FROM the circumstance that nearly all their skeletons found in the English Lias have a dislocation in the vertebræ of the tail, Sir Richard Owen was led many years ago to the conclusion that the extinct marine reptiles, known as Ichthyosaurs, were furnished with an expanded fin at the end of the tail, and that the weight of this fin caused the fracture in question. Years passed on without affording any evidence one way or the other in relation to this bold theory; but at length, in the present year, there has been discovered in the Lias of Württemberg the skeleton of one of these reptiles, in which the outline of the fleshy parts is completely preserved, and which proves the existence of a caudal fin of still larger dimensions than Owen supposed to be the case. This interesting specimen is described by Dr. E. Fraas in the paper standing fourth in our list, from which the accompanying figures are reproduced. We already knew that in the paddles the fleshy part was extended much behind the bony skeleton; but the new specimen shows us that, in addition to the tail-fin, the Ichthyosaurs had a triangular fin on the middle of the back, behind which was a crest of horny excrescences compared to those of the crested newt. The tail-fin is vertical and nearly symmetrical externally, although the backbone runs downward to terminate in its lower lobe. In this respect the fin has the same general aspect as in the Sharks, except that in the latter the backbone runs into the upper lobe. It shows, indeed, as Dr. Fraas remarks, how closely analogous is the form of Ichthyosaurs to fishes; and it thus further justifies the title of "fish-lizards" which has been applied to these reptiles.

An exhaustive comparison of the skeletons of the Ichthyosaurs from the Liassic deposits of the Continent with those from the corresponding horizons of England, has long been a desideratum. This want has, however, now been remedied by Dr. Fraas, in the magnificent work mentioned second in the following list. By no means the least important features in that work are the beauty, number, and large size of the plates with which it is illustrated; and with these by his side the English Palæontologist ought no longer to have much

doubt as to the nature of the relationships of the Continental to the British Ichthyosaurians.

It need hardly be mentioned that the first Ichthyosaurs specifically described were obtained from the Lower Lias of our own country, and there is consequently no sort of doubt as to the names by which they should severally be designated. Very different, however, is the case with the species rom the Upper Lias ; names having been applied to specimens in this country and on the Continent, many of which there has long been reason to regard as specifically identical, although there has hitherto been great difficulty in obtaining decisive evidence on these points. Dr. Fras has, however, now enabled us to judge for ourselves on these difficult questions, and has brought

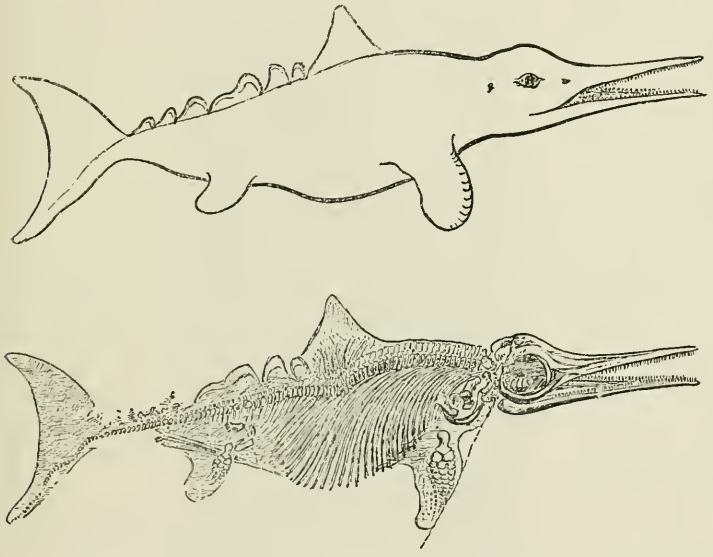


FIG. 1.—Skeleton of an Ichthyosaur, showing the outline of the soft parts, together with a restoration of the same. (*From Fraas.*)

forward evidence to show that there is every probability of the identity of the majority of the British and Continental species.

Dr. Fraas commences his memoir with an interesting survey of the history of the gradual acquisition of our knowledge of the structure of the Ichthyosaurian skeleton, from the date of Sir Everard Home's first memoir published as far back as 1814. We may observe, however, that when stating that the name *Ichthyosaurus* is antedated by *Proteosaurus* of Home, the author appears to have been misled by a similar statement in the British Museum Catalogue of Fossil Reptiles ; the real fact being that the former name is three years the earlier of the two.

The historical portion of the memoir is succeeded by a dissertation on the general structure and affinities of the Ichthyosaurs. Here the author heartily endorses the views of Dr. Baur as to the close

affinities of these extinct reptiles with the primitive Rhynchocephalians (of which the sole remaining representative is the Tuatera Lizard—*Sphenodon*—of New Zealand). Dr. Fraas is, however, by no means thereby prepared to accept the view that Ichthyosaurs, Rhynchocephalians, and Lizards should be regarded as constituting one great branch of the reptilian tree. Seeing, however, that he admits the probable origin of these reptiles from a primitive Rhynchocephalian, the cause of his hesitation to adopt this view is not very apparent. That the Ichthyosaurs have sprung from a stock which was originally terrestrial, and hence occupy a position among reptiles precisely similar to that held by the Whales among mammals, Dr. Fraas fully endorses.

In regard to the author's *résumé* of the structure of the Ichthyosaurian skeleton, we have but one point to mention. This relates to the bones forming the palate, in regard to the interpretation of which there has been some difference of opinion. While adopting the view taken in the British Museum Catalogue of Fossil Reptiles, that the pair of bones occupying the middle of the palate in advance of the pterygoids are the vomers, Dr. Fraas, as shown in plate ii. of his memoir, considers that the bony bars flanking the two latter severally consist of an anterior moiety identified with the palatine, and a posterior moiety corresponding with the transpalatine or transverse bone. It is true that in the skull figured by Dr. Fraas from Sir R. Owen's plate there appears to be a division in this bar, but we strongly suspect this is due to fracture; and there is none in the type skull of *I. zetlandicus* figured by Professor Seeley. Moreover, the position of the palatines entirely in front of the posterior nostrils would be quite abnormal. We have accordingly no doubt but that the whole of these bony bars are the palatines.

Many of the considerations relating to the probable mode of life of the Ichthyosaurs referred to by our author cannot fail to command attention; but the supreme interest attaches to the evidence in favour of the viviparous reproduction of at least some of these reptiles. It has been long known that certain large skeletons of Ichthyosaurs from the Upper Lias of Holzmaden, in Württemberg, and elsewhere, are found with the skeletons of one or more much smaller individuals enclosed partly or entirely within the cavity of the ribs as shown in our figure. Of such skeletons there are four in the museum at Stuttgart, two in that of Tübingen, one at Munich, and others in Gent and Paris (Fig. 2). Of these, two in Stuttgart, as well as the two in Tübingen, contain but a single young skeleton, while one of those at Stuttgart has four, the Munich specimen five, and the remaining Stuttgart example upwards of seven young. Some of these young and, presumably, fœtal Ichthyosaurs have the head turned towards the tail of the parent, while in others it is directed the other way. That these young have not been swallowed by the larger individuals within whose ribs they are found, is pretty

evident from several considerations. In the first place their skeletons are always perfect. Then they never exceed one particular size, and always belong to the same species as the parent. Moreover, it would appear to be a physical impossibility for one Ichthyosaur of the size

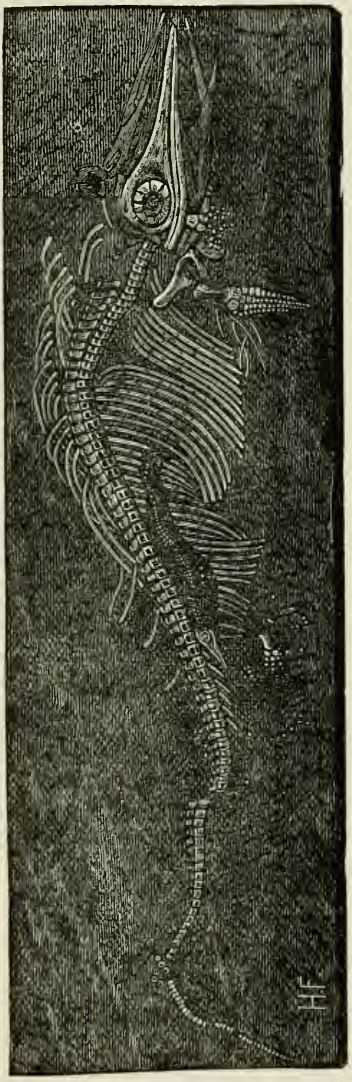


FIG. 2.—Skeleton of *Ichthyosaurus quadriscissus*, with fœtus enclosed in the body-cavity. $\frac{1}{4}$ natural size. (*After Gaudry.*)

of the Stuttgart specimen to have had seven smaller ones of such dimensions in its stomach at one and the same time. We may accordingly take it for granted that these imprisoned skeletons were those of fœtuses. It is, however, very remarkable that, so far as we

are aware, all the skeletons with fœtuses belong to one single species; thus suggesting that this particular species was alone viviparous. Dr. Fraas considers that the retention of the ova within the body of the parent until developed into fœtuses of considerable size, is to be accounted for by the purely marine mode of life of these reptiles, which he believes were not in the habit of visiting the shore. If it should prove that viviparous reproduction was confined to the species with narrow paddles, like the one represented in our figure, it may be suggested that the species with broad paddles, like *I. communis* of the Lower Lias, were accustomed to visit the land to deposit their eggs, and thus had no need of special arrangements in regard to their reproduction.

Coming to the systematic portion of the memoir, we find, as might have been expected, that the small Ichthyosaur from the German Trias, described as *Ichthyosaurus atavus,* is transferred by Dr. Fraas to the genus *Mixosaurus*, originally founded upon the evidence of specimens from the Italian Trias. This genus, it may be observed, in the structure of its limbs, tends to bridge the gap between the true Ichthyosaurs of the Lias and more normal reptiles; and it is regarded by Dr. Fraas as the proximate ancestor of the two great groups of Ichthyosaurs, namely, those with narrow and those with broad paddles. Of the Ichthyosaurs of the Lower Lias, Dr. Fraas has but little to say, since they are but poorly represented in Germany. We may observe, however, that it has been shown in the "Catalogue of the Dublin Museum," standing sixth on our list, that the long-snouted forms described as *I. tenuirostris* and *I. latifrons*, really belong to but a single species, for which the former name must be employed. From our own studies we also fail to find satisfactory means of distinguishing the long-snouted Ichthyosaurs of the Upper Lias from the Lower Liassic species, and we are, therefore, fain to regard the three species, *I. tenuirostris*, *latifrons*, and *longirostris*, admitted by Dr. Fraas, as founded upon one and the same form. Our readers desirous of further information on this species may refer to the paper standing last on our list.

The great bulk of the descriptive portion of Dr. Fraas' memoir relates to the Ichthyosaurs of the Upper Lias, the skeletons of which are found so beautifully preserved in the shales of Holzmaden. For the common German species (the one represented in our figures) the name *Ichthyosaurus quadriscissus* (in allusion to the slits in the edges of the bones of the front of the paddles) is adopted; and, on the whole, we think this is the best course, seeing that our author has shown that *I. acutirostris*, with which this species had been identified, is a totally distinct form. This species is represented in the Upper Lias of England as well as Germany; and Dr. Fraas is disposed to consider that the huge English Ichthyosaur, described as *I. zetlandicus*, together with an identical form from Normandy, figured as

I. longifrons, is only a variety of this species, an opinion with which we are much disposed to concur.

Much important information is furnished by Dr. Fraas with regard to certain Ichthyosaurs of the Continental Upper Lias, described by Theodori in 1854, which it has hitherto been impossible for English writers to refer to their proper systematic position. The first of these is *Ichthyosaurus crassicostatus*, which is now shown to be allied to *I. tenuirostris*, although having a much shorter snout. It has, therefore, the same relation to the latter as is presented by the so-called *I. brevirostris* to *I. communis* of the English Lower Lias. We are, however, by no means assured that such short-snouted forms—which, be it observed, are of extreme rarity—are anything more than abnormal examples of the longer-snouted species to which they are severally allied, similar abnormalities being well-known to occur among the Cetacea. Then, again, we have the still more rare *I. macrophthalmus*, which appears to differ from the type of *I. crassicostatus* only by its larger orbits, smaller teeth, thinner ribs, and the absence of slits in the bones of the paddles. In respect to its teeth, this form serves, indeed, to connect *I. crassicostatus* with *I. quadriscissus*, and it thus appears to us to require much stronger evidence than any yet published to prove that these three forms are really specifically distinct.

The third of these hitherto imperfectly known Upper Liassic forms is *I. ingens*, which is now definitely referred to the Tenuirostrine group, and appears to be mainly characterised by its gigantic size. In this respect it, however, scarcely exceeds the type of *I. zetlandicus*; and we see no reason why it should not be specifically the same, in which case Theodori's name would have to be retained. Moreover, if this form be identical with *I. zetlandicus*, and the latter with *I. quadriscissus*, the name *I. ingens* antedates both the others; the same being the case with regard to the names *I. crassicaudatus* and *I. macrophthalmus*.

Before leaving the Upper Liassic Ichthyosaurs of the Tenuirostrine group, we must briefly refer to the paper by Professor Gaudry, quoted at the end of this review. This refers to a large Ichthyosaur discovered in the Upper Lias of the Yonne, and provisionally named *I. burgundiæ*. It is described as having a long snout, with a pectoral limb of the type obtaining in the Tenuirostrine group, and the teeth fluted. The pelvic limb is, however, peculiar, in that it has two rows of bones arising from the intermedium, or median bone of the tarsus, after the fashion obtaining in *I. communis*; and it appears to be this character which led Professor Gaudry to consider the specimen as indicating a new species. We would suggest, however, that such a peculiarity may perfectly well be an individual one, in which event we fail to see that the other points mentioned in the description are sufficient to distinguish this skeleton from *I. zetlandicus*. The specimen is, however, of considerable interest, as showing how species

with narrow paddles (*Longipinnate*) may have passed into those in which the paddles were wide (*Latipinnate*).

With *Ichthyosaurus acutirostris* we come to the first of those species in which the teeth, instead of being evenly fluted all round, are smooth, and carry two or three sharp vertical ridges; from which character the members of this group have been separated from the genus *Ichthyosaurus* as *Temnodontosaurus*. Dr. Fraas has rendered good service in showing that *I. acutirostris* is a member of this group, and consequently has nothing to do with *I. quadriscissus*, with which it had been erroneously identified. The only other Upper Liassic species mentioned is the gigantic *I. trigonodon*—the analogue of the still more gigantic *I. platyodon* so commonly found in the Lower Lias of England.

Of the post-Liassic species of the Jura, the materials at Dr. Fraas' command do not appear to have been very abundant. It is, however, considered that the Jurassic *I. leptospondylus* is an ancestral type of the genus *Ophthalmosaurus*, characterised by having three bones articulating with the humerus, and being evidently the extreme modification of the Latipinnate type; a further characteristic being the tendency to a partial or complete loss of the teeth. On the other hand, our author is indisposed to admit the reference of the well-known Cretaceous *I. campylodon* to the Latipinnate group, and rather considers that this powerfully-toothed species has been derived from the Longipinnate group, which is not regarded as the ancestral stock of the Latipinnates. The main ground of this new departure appears to be the alleged resemblance in the structure of the skull of *I. campylodon* to that of the Longipinnates, as displayed in sections. No explanation is, however, offered to account, on this view, for the resemblance of the radius of the Cretaceous species to that of the Latipinnates, and its dissimilarity to the corresponding bone of the Longipinnates. Some day, perhaps, we may have the good fortune to obtain an entire paddle of *Ichthyosaurus campylodon*, by which the question will be decided finally. With a brief reference to Professor Capellini's memoir, recording the occurrence of this widely-spread species in the Cretaceous of Italy, we may appropriately bring to a close our survey of these important series of contributions to our knowledge of the Ichthyosaurs.

REFERENCES.

1. **Capellini, G.**—Ichthyosaurus campylodon nelle Argille Scagliose dell' Emilia. *Mem. Accad. Ist. Bologna*, ser. 4, vol. x., pp. 431–450, pl. i., 1890.

2. **Fraas, E.**—Die Ichthyosaurier der Süddeutschen Trias- und Jura-Ablagerungen. 4to, Tübingen, 1891, pp. 81, pls. xiv.

3. ——— *Ichthyosaurus numismalis. Jahreshefte Vereins vat. Nat. Württ.*, 1892, pp. 22–31, pl. i.

4. ——— Ueber einen neuen Fund von *Ichthyosaurus* in Württemberg. *Neues Jahrbuch f. Mineralogie*, 1892, vol. ii., pp. 87–90.

5. **Gaudry, A.**—L'Ichthyosaure de Sainte-Colombe, *Comptes Rendus*, vol. cxiii., pp. 169–172, 1891.

6. **Lydekker, R.**—Catalogue of Fossil Mammals, Birds, Reptiles, and Amphibians in the Science and Art Museum, Dublin. 8vo, Dublin, 1891, pp. 61.

7. ——— Note on a nearly perfect Skeleton of *Ichthyosaurus tenuirostris* from the Lower Lias of Street, Somerset. *Geol. Mag.*, 1891, pp. 289–290, pl. ix.

R. Lydekker.

V.

Notes on the Development and Structure of Arachnids.

IN a recent contribution to NATURAL SCIENCE (pp. 281, 2) I discussed the correspondence of the appendages of Insects and Arachnids in connection with Cholodkowsky's researches on the embryology of the cockroach. It will be remembered that he maintains the post-oral origin of insect antennæ and their consequent homology with the cheliceræ of spiders, scorpions, and their allies. In this he is at variance with most modern writers, who, regarding the antennæ of insects as arising from the pre-oral region of the embryonic head, compare the cheliceræ of Arachnids with the mandibles of Insects. The former, however, was the prevalent view some years ago, and is restated quite recently in the elaborate Memoir (1) on the Anatomy of Arachnids, by Gaubert, who, on this point, follows his master, Milne-Edwards.

But Gaubert refers to the other view (that of Balfour), and admits that considerable support has been lately given to it by the researches of Jaworowski (2, 3). The latter observer has studied the embryology of a large "tarentula" or hunting spider (*Trochosa signoriensis*), and he describes and figures a pair of structures which arise behind the mouth and in front of the cheliceræ, which never become jointed, and which ultimately sink into cavities in the blastoderm. These he regards as rudimentary antennæ. In front of the segment to which they belong, he describes and figures three other segments, of which only one appears to be pre-oral. By adding these four segments to the six which bear the appendages which persist in the adult spider, we see that the cephalothorax is formed by the fusion of at least ten segments. Though no previous observer has described antennæ in an embryo Arachnid, Jaworowski points out the similarity between the rudiments figured by him and a pair of structures, which are shown in exactly the corresponding position in the figure of the embryo of *Agelena labyrinthica* in Balfour's "Treatise on Comparative Embryology" (vol. i., fig. 200*d*). A comparison between the two figures[1] makes it highly probable that

[1] Jaworowski's figures were reproduced in *Nature*, vol. xlvi., 1892, pp. 272, 3, to illustrate Professor Sollas' review of Simroth's "Die Entstehung der Landtiere."

the structures are homologous. Now in the "Treatise" they have no reference-letters, and it might be inferred that Balfour, though he saw and figured rudimentary antennæ, failed to recognise their importance. But if we turn to his original paper (4), from which the figure in question was taken, we find that he describes these structures, without doubt, as the ectodermal thickenings from which are developed the ganglia innervating the cheliceræ. Is it possible, then, that Jaworowski has mistaken these thickenings for appendages, and that the supposed antennæ are in reality only incipient nervous centres? The fact that they sink into cavities seems to lend support to such a view, but their pointed appearance (they are more pointed than in Balfour's figure) suggests appendages. Probably we shall be correct in assuming that the antennæ of the ancestors of the Arachnids arose in the region where the ganglia were being developed, and that in some forms, as *Trochosa*, the pointed rudiments indicate this, though their subsequent fate is to form part of the nervous centre.

But, whatever may be the exact meaning of these rudiments, the presence of three segments between the cheliceræ and the mouth seems certain. And so, while Jaworowski supports Cholodkowsky's view of the post-oral origin of insect-antennæ, it is impossible to regard them as equivalent to the cheliceræ of spiders; for the antennæ are said to arise from the first post-oral segment, while the cheliceræ are developed from the fourth. The presence of certain transitory appendages in front of the mandibles in insect-embryos, taken in connection with the segments observed by Jaworowski in front of the cheliceræ in the embryo of *Trochosa*, confirms the view that the mandibles in the one class correspond with the cheliceræ in the other; and working backwards along the appendages, we identify the maxillæ of Arachnids with those of Insects, the first pair of legs in Arachnids with the second pair of maxillæ, which form the lower lip of Insects, and the second, third, and fourth pairs of legs in Arachnids with the three pairs of insect-limbs. We conceive that the two classes have diverged from a common stock; in the Arachnids the antennæ have been lost, the head has undergone fusion with the thorax, and two pairs of limbs only have the function of jaws; while in the Insects the head has been highly specialised, the antennæ preserved, and three pairs of limbs pressed into the service of the mouth. The passage between a walking-limb and a jaw is well shown in the harvestmen (Phalangidea), in which the first two pairs of legs have at their bases small masticatory plates. It is highly interesting to note how, in various groups of Arachnids, the place of antennæ is supplied by other limbs; in the spiders by the maxillary palps or by either of the front two pairs of legs, in the spider-scorpions (Pedipalpi) by the wonderfully attenuated first pair of legs.

When we consider that the insect and arachnid types were already developed in the Silurian period, we can hardly expect that

palæontology will ever throw much light on the ancestry of the two classes; it is to the structure and development of living forms that we must look for our information.

In the embryo of *Trochosa* the limbs, which afterwards become so differentiated as the cheliceræ, and also the rudimentary limbs on the abdominal segments, which afterwards disappear altogether, show indications of jointing like the legs; this is strong evidence of the former ambulatory function of all the limbs. Another observation of great interest made by Jaworowski is that in the cephalothoracic appendages of the embryo *Trochosa* (except the cheliceræ) there are parts which correspond to the coxopodite, endopodite, exopodite, and epipodite of a crustacean limb. The epipodite is more obscure in the hinder segments, and, in all cases, disappears. From the fact that the epipodite of a crustacean often bears a gill, and that the spider's epipodite is a transitory embryonic structure, Jaworowski infers the descent of the air-breathing Arachnids from water-breathing ancestors. The epipodite of *Trochosa* is only mentioned in his most recent paper (3). It is rather amusing that Simroth in his book, "Die Entstehung der Landtiere," should have used Jaworowski's first paper (2) with the description of the endo- and exopodite of *Trochosa* in support of the exactly opposite conclusion: that water-breathing crustaceans have branched off from ancestors that lived on the land!

The aquatic representative of the Arachnids, however, is the King Crab (*Limulus*), whose gills, borne on the hinder faces of the flattened abdominal limbs, have been repeatedly compared with the lung-books of spiders and scorpions. Lankester (5) and Kingsley (6) suggested the formation of the arachnid lung-book by the invagination of an appendage intermediate in form between the gill of *Limulus* and the pecten of a scorpion. MacLeod (7) proposed that the flattened appendage of *Limulus* might become fused by its edges with the body-wall, so as to enclose the gill-lamellæ. Laurie in a recent paper (8) inclines to the latter view. He finds that in the embryo of *Scorpio fulvipes* the lamellæ lie horizontal, and parallel to the ventral surface, directed forwardly and outwardly from the stigmatic opening. Such an arrangement would require the invagination of an appendage bearing the lamellæ on its front face, whereas the appendages of *Limulus* bear them on the hinder face, and so Lankester's theory would require the complete reversal of the gills. No such difficulty is raised by MacLeod's view. It is noteworthy that in the adult *Scorpio* the lamellæ take up a different position from what they have in the embryo, lying vertically and parallel to the axis of the body. In the adult spider they retain the horizontal position, so that in this respect the spiders show the more primitive arrangement.

In most Arachnids a pair of coxal glands is found near the base of one or more of the pairs of legs; these structures, which open to the exterior only in the embryo and become ductless in the adult,

are generally regarded as the surviving representatives of a series of nephridia, which formerly occurred in nearly all the segments, as in many worms and in *Peripatus*. Lebedinsky has recently (9) described the development of these glands in the common harvestman (*Phalangium opilio*) in the embryo of which they open behind the first pair of legs. Each consists of a coiled tube with thin walls opening to the exterior by a straight duct with thicker walls. This duct, as well as the glandular portion, is of mesodermal origin, being formed by evagination, not, as in nephridia generally, by invagination of the ectoderm. At present, Lebedinsky prefers to call these coxal glands of Arachnids, and the shell- and green-glands of Crustaceans "homodynamous" rather than "homologous."

Passing from embryology, we have, in the work of Gaubert (1) already referred to, an important contribution to the minute anatomy of adult Arachnids of various orders. A comparative review is given of the chitinous integument, chitinogenous layer, glands, sense organs, appendages, locomotion, and influence of turgescence on the movements of joints. Gaubert has only observed two layers in the cuticle, and consequently doubts the existence of the third (intermediate) thin layer described by Schimkewitsch. He has made experiments on the cause of the rupture of the cuticle when it is cast, and has produced artificial ecdysis by plunging spirit-specimens of spiders into boiling water, concluding from this that the rupture is simply due to mechanical pressure caused by the increased size of the animal.

Elongated cells with large nuclei, occurring in the hypoderm of the jaws, and believed by some former observers to be nervous, are stated by Gaubert to be glandular, and to secrete a sticky fluid which mixes with that from the maxillary glands; similar cells occur on the lower lip, and in the pharynx. Glandular cells also form the hypoderm at the base of the femora in *Phalangium*. The two layers of the cuticle and the hypoderm are continued inwardly into the tendons, and outwardly into the hairs, spines, and claws; so that all the three latter have a similar structure. This fact should prove consoling to beginners in Arachnid classification, who may be often puzzled to decide whether a certain excrescence is a "hair" or a "spine."

Gaubert devotes much space to the "lyriform organs" on the cephalothorax and limbs of Arachnids. These consist of series of fine slits in the cuticle with central circular enlargements, filled by thin bands. A number of these slits side by side give somewhat the appearance of a stringed instrument, hence their name. They were discovered on spiders by Bertkau, and now Gaubert has found them also on spider-scorpions, harvestmen, and pseudoscorpions. He gives very full descriptions of their numbers and position on the numerous forms he has examined. From the enlargements of the thin bands nerve fibres pass inwards, which lead to large nerve-cells, whence fibres pass on to the nerve-trunk. The function of the lyriform organs was believed by Bertkau and others to be auditory.

Gaubert rejects this view, on the ground that there is no satisfactory evidence that Arachnids can hear at all. He covered the regions where the organs occur in several spiders with a light coat of varnish, and put these, together with uninjured specimens, into one end of a bottle, which he heated; the spiders which had been varnished did not try to escape from the heat as soon as the others. From this experiment, he concludes that the function of the lyriform organs is that of appreciating differences in temperature, "et peut-être aussi d'autres sensations générales." This result can hardly be regarded as conclusive; the closing of the slits by a coat of varnish would, of course, diminish the creature's power of feeling heat, but this does not prove that the organs have not another and more specific use. The thin bands certainly suggest the reception of vibrations as their function. The experiments of the Rev. O. P. Cambridge ("Spiders of Dorset," p. 582) on *Epeira diadematata* seem conclusively to prove that that species, at least, can hear. Moreover, the presence of stridulating organs in certain male spiders points decisively to a power of hearing in their females.

Much attention has been paid by Gaubert to the pecten of scorpions; he describes in detail these organs in *Buthus*. They are worked by a complicated system of muscles, turning the whole appendage, and also moving the teeth. The cuticle of the teeth is raised into numerous papillæ, each having a pore filled by a nerve-ending. The function of the pecten seems to be undoubtedly tactile, with special reference to copulation. The sense-organs on the palps and first pair of legs of *Galeodes*, described by Bertkau (*see* NATURAL SCIENCE, No. 1, p. 53), are independently described by Gaubert, who regards them as possibly auditory. The varying functions ascribed to such organs by different observers show how much we still have to learn of the life of Arthropods.

The structure of the appendages and their muscles in the various orders of Arachnids is described in great detail, and the homology of their joints carefully worked out; the attachment of the muscles is of great value in this respect. According to Gaubert, the limb of an Arachnid has three main divisions: a proximal part, directed upwards, and consisting normally of coxo-, basi-, and meropodite (coxa, trochanter, and femur), a horizontal part, sometimes of a single joint, sometimes divided into two, carpo- and propodite (genua and tibia), and a distal downwardly-directed part, the dactylopodite, which is also sometimes divided into two (meta-tarsus and tarsus). An extra division may be developed in any of these three parts of the limb; in the spiders and harvestmen the horizontal part is divided into genua and tibia, permitting lateral motion; in the Solfugidea and pseudo-scorpions there is an extra joint between the trochanter and the femur; while in the scorpions the distal part of the limb is divided into three joints, the horizontal part remaining undivided. It thus appears that, although the limbs of a spider and a scorpion each have

seven joints, only the first three in each are homologous. The palp of a female spider or a harvestman, in which the horizontal and distal parts are alike undivided, represents the primitive Arachnid limb.

In walking, an Arachnid moves nearly together the first and third legs on one side with the second and fourth on the other, according to the observations of Gaubert. Several interesting contributions to our knowledge of the locomotion of the Arthropoda have recently been made, but a consideration of them must be reserved for a future occasion.

REFERENCES.

1. **Gaubert, P.**—Recherches sur les Organes des Sens et sur les Systèmes tégumentaire, glandulaire et musculaire des appendices des Arachnides. *Ann. Sci. Nat. Zool.*, vol. xiii., 1892, pp. 31–184, pls. i.–iv.

2. **Jaworowski, A.**—Ueber die Extremitäten bei den Embryonen der Arachniden und Insecten. *Zool. Anz.*, vol. xiv., 1891, pp.164–9, 173–6.

3. ——— Ueber die Extremitäten, deren Drüsen, und Kopfsegmentierung bei Trochosa signoriensis. *Zool. Anz.*, vol. xv., 1892, pp. 197–203.

4. **Balfour, F. M.**—Notes on the Development of the Araneina. *Quart. J. Micro. Sci.*, n.s., vol. xx., 1880, pp. 167–189, pls. xix.–xxi.

5. **Lankester, E. Ray.**—A new Hypothesis as to the Relation of the Lung-book of Scorpio to the Gill-book of Limulus. *Quart. J. Micro. Sci.*, n.s., vol. xxv., 1885, pp. 339–42.

6. **Kingsley, J. S.**—Notes on the Embryology of Limulus. *Quart. J. Micro. Sci.*, n.s., vol. xxv., 1885, pp. 521–76, pls. xxxvii.–ix.

7. **MacLeod, J.**—Recherches sur la Structure et la Signification de l'Appareil Respiratoire des Arachnides. *Archiv. Biol.*, vol. v., 1884, pp. 1–34, pls. i.–ii.

8. **Laurie, M.**—On the Development of the Lung-books in Scorpio fulvipes. *Zool. Anz.*, vol. xv., 1892, pp. 102–5.

9. **Lebedinsky, J.**—Die Entwicklung der Coxaldruse bei Phalangium. *Zool. Anz.*, vol. xv., 1892, pp. 131–7.

Geo. H. Carpenter.

VI.

Death in the Forest.

THE tropical forest is the scene of an intense struggle for life. Undisturbed by man, the trees, climbers, and epiphytes are always fighting for their share of sunlight. The weakest, of course, succumb, making a little more room for the others; and the close observer is painfully impressed by signs of disease and death on every hand. What is most striking is the fact that a majority of the deaths are the result of smothering, blood-sucking, and strangling by other plants. Like men in great cities, each plant is an individual trying his best for himself, no matter what becomes of the others. But there is no one to help the weak or nurse the sick. They must be able to cope with their neighbours or must die. Once weakened, their places are usurped by others, and recovery is impossible.

The loss is greatest in early life. Animals and savages take care of their children in their weakest stage, but not so the forest giant. His seeds are scattered, provided with contrivances to spread them in every direction by means of wind, water, or animals, but beyond this they receive neither care nor attention. Thousands of seeds are produced annually, but rarely does one survive and come to maturity. They germinate in every direction, some growing upwards for a few feet, only to give up the struggle when they find no light is obtainable. In the dull arcades of the forest perpetual twilight reigns, and the seedlings can get neither strength nor colour. They droop and wither, some almost immediately, others in a few months, while hundreds fail after gaining a fair position. They cannot pierce the dense canopy above, and are virtually smothered to death.

Between the tree trunks the ground is littered with fallen leaves, petals, and fruits, crumbling down to that rich brown mould so characteristic of the forest. Except a few voyrias or a saprophytic orchid, not a single herbaceous plant is to be found. Like the tree-seedlings, they cannot exist without sunlight. As in the case of the cave-fish, however, the voyria[1] has managed to live under conditions decidedly unfavourable to others, and even to itself, as may be seen

[1] A small leafless plant, of the natural order Gentianeæ, growing on rotting wood and leaves in the forests of tropical Africa and America.

from its weak, leafless, and almost colourless stem. One species is nothing but a stalk supporting a single flower, like a miniature yellow crocus, while others, with branching stems and several flowers, are even more insignificant.

When a clearing is made, however, the result is almost marvellous. In a month or two it is green with herbage, and if left alone will soon become as densely packed as the roof of the forest. Various kinds of weeds, of species unknown in the neighbourhood before, now dispute with each other possession of the vacant space. It is almost impossible to conceive how the seeds have been brought, as it may be scores of miles to the next clearing where the vegetation is identical. Some, no doubt, have been left by floods, others dropped by birds, or carried by the wind. One of the first to appear is the elegant silver fern (*Gymnogramme calomelanos*) which particularly affects the sides of ditches and every spot where the soil has been turned up. After the ferns come grasses and sedges, then phytolaccas, prickly solanums, and a host of interlacing scrambling vines, until an impenetrable jungle extends over the whole clearing. The struggle that goes on here is intense, and the weakest are soon smothered. The first to give up are the ferns, then follow the grasses, while the prickly solanums dispute every inch with the smothering multitude, scratching and tearing the leaves and stems of the scramblers as if determined not to give way. But all their efforts are useless; the smotherer throws its arching stems over them and on these others lay hold, until a dense mass of foliage shuts out the light from everything below.

Meanwhile seeds of forest trees have found the opening. Without the accident of a clearing they would have gone the way of the myriads that die for want of such an opportunity. But they will have a hard struggle, for already the smotherers are doing their best to prevent anything from growing beneath them. However, one after another they pierce through the tangle, taking advantage of every little crevice, and often growing through those leaves they cannot push aside. Once out in the light they begin to spread and overshadow the choking creepers, which lose their leaves and become less hampering. Here and there one tries to lay hold of a young tree and rise with it, but rarely does it succeed, as there are no branches and the leaves are thrown off one after another as the slender trunk rises upwards. Soon the clearing looks like a young plantation so densely packed as to be almost impassable. The mass of creepers is now nothing but a network of dry stems, which are gradually settling down into the soil.

The young trees now form a crowd so dense that no one has room to spread. The space required by a single tree is occupied by a dozen or more, and something must be done. Then comes the final effort. The slightest advantage of one over another, in strength, height, or expanse of foliage, determines the result, which means

death to the many and conquest to the few. As those which have got ahead stretch themselves over the others the sunlight is excluded and death is only a matter of time. Sometimes the struggle between two trees is a very close one. They are apparently equal in size and height, and as they interlace their branches, there is a continual straining to get ahead of each other. One raises a branch above in one direction, while its antagonist succeeds in covering it in another, and so the fight goes on, neither gaining the advantage or being able to arrive at an assured position.

As may be seen, the difficulties in the way of a seed or young forest tree are enormous. Millions of fruits ripen only to become food for labbas and acouries on the ground, or birds and monkeys in the trees. Most of those in Guiana are light enough to float on the water, and on account of this provision are carried away by floods and deposited at great distances from the parent trees. But the forest is crowded—it cannot find room for more. Unless a vacancy occurs there is no chance for a newcomer. And when this happens, what a struggle for the position! Hundreds of candidates are on the ground, and there is no patronage or favouritism unless man interferes. The one that succeeds may perhaps be the most worthless in the eyes of the woodcutter. . A soft-wooded tree, for instance, will grow more quickly and outstrip the others. Various contrivances have been adopted. It seems as if everything has been tried, but rarely have two species chosen the same means to the end. All the trees of the Guiana forest have, however, agreed to avoid branching on the lower part of the stem. If branches are produced in early life, these are very small, and soon drop off so as not to interfere with their upward growth. Every effort is made to reach the common level, while to support the slender trunk and prevent its becoming top-heavy, buttresses are thrown out at the base.

It would seem that if once the forest were occupied, there could never be the slightest chance for another tree, without the interference of man. Such would actually be the case were there no destructive agencies. Once over the drawbacks of infancy, its head on a level with those of its neighbours, the forest giant would have merely to maintain its position. But in reality it has to contend with a number of insidious foes. Here and there in the forest fallen trunks, with no sign on them of axe or other instrument, lie along the ground, and crumble slowly away under the influence of moisture and the attacks of wood-ants and beetle larvæ. Occasionally, the traveller comes upon the scene of a recent downfall. A tree has partly fallen, producing a break in the continuity of the forest, as well as a confused mass of wreckage. In the line of its course, are ragged ends of great limbs, twisted branches, headless palms, and torn bush-ropes. As yet it is partly supported by the mass of foliage, branches, and trunks of other trees which stand in the way, but as these decay it will gradually sink until it becomes a rotting log on the ground.

Rarely does such an accident happen to a healthy tree. When nothing has occurred to weaken it, the branches interlace with those of others in such a manner that a fall is almost impossible. Wood-cutters have often to clear away several trunks before they can fell the one they require. The tree is almost invariably dead before it falls naturally. Perhaps, every branch has become dry and decayed, and then broken away little by little, until nothing remains but a bare stem. The roots also have rotted in the ground and no longer serve as anchors, so that all support is lost at both summit and base. Then comes a heavy rain-fall or flood, and the dead trunk crashes down, carrying away everything in its course.

In paddling up some of the smaller rivers of Guiana dead and dying trees may be seen on every side. In various stages of decay, some straining to keep alive a branch or two, they provide hollows for parrots to build in, and food for the wood-ants, which riddle them with their tunnels, and hang their oval black nests in the forks. Here are the remains of magnificent trunks over a hundred feet high, with bare stems and a few ragged strips of bark, only kept from falling by bush ropes and parasites. Perhaps one has fallen across the creek, seriously impeding its navigation. Festooned with creepers, they do not look so unsightly as might be supposed; while large bushes perched in the topmost forks form, as it were, a flowery pall. Some of the most handsome are the clusias, with their smooth stem and shining leaves, and flowers like single camellias. Some have petals a pure white, others tinted with rose-colour. Yet, with all its beauty, this plant is a deadly foe to the forest tree. In Jamaica it is called the "Scotch attorney," that gentleman being proverbially noted for working plantations to his own profit.

Clusias are not the only stranglers; different kinds of wild figs are quite as destructive. These are also clean and sleek in limb and foliage, but have no showy flowers. Their fruit varies in different species from the size of a pea to that of a cherry, and is greedily devoured by birds. Of a rich dark green, their clumps contrast favourably with the decrepit trees on which they sit, and which they appear to hold together by interlacing aerial roots.

Unable to fight for themselves in the struggle, these stranglers have become established in the forks of the trees, where the birds have dropped their seeds in cracks of the bark. Here, among mosses and in congenial positions, they soon germinate and insinuate their aerial roots into crevices and among the damp epiphytes, adhering very closely to the tree, but deriving no nourishment from it. Slowly they creep down towards the earth, broadening as they lengthen, while branches and leaves are developed above. To all appearance they are quite harmless, apparently taking advantage of their host merely to get a share of the sunlight, and, like the thousands of epiphytic orchids, bromelias, arums, ferns, and mosses already resting securely on their immense limbs, causing no injury. Never-

theless, these climbers are preparing for a struggle in which the apparently weak will overcome the strongest monarch of the forest. If the seed has germinated in a suitable fork, its aerial roots flow down on every side, like narrow streams of green pitch, accommodating themselves to every irregularity, and only stopping when they reach the earth, and push their roots into the rich mould. Then the little plant becomes a large shrub, day by day increasing in strength, until, by its dense shadow, it kills every epiphyte in its neighbourhood. Then the aerial roots enlarge until they resemble monster pythons hanging downwards instead of coiling round the tree, which now begins to feel the power of its enemy. From every clinging root side-roots are developed, which unite until, in some places, the whole trunk is encircled.

In forest trees the seat of life is just inside the bark, where, as is well-known, the growth of a season is shown by a ring of new wood. Such trees can be killed by ringing the bark with an axe, or by constriction at the base, and it is by the latter process that the clusia or fig performs its murderous work. As it becomes more luxuriant and tightens its bands, the flow of sap is checked and the leaves begin to fall off. This is greatly to the destroyer's advantage, as it could not get enough light while every inch was occupied by the canopy of foliage. Unlike many epiphytes, it enjoys the blazing sunlight and has developed all its powers to attain a position where this may be secured. Its leaves are thick in texture, and even the flowers of the clusia do not shrivel in the sun like those of most plants. Few forest trees are so well fitted to endure the intense glare, none look so strong and healthy under the influence of a cloudless sky, and the consequent drought for two or three months.

Leaf after leaf falls, until the tree becomes too weak to ripen seeds and bears but few flowers; soon nothing is left but bare twigs. That it is struggling in the embrace of its enemy may be seen by the way the bark swells between its interlacing bonds. Like an athlete straining to break the cords which bind him, the tree tries to rid itself of the living fetters. All its efforts are, however, as useless as those of an ox in the python's coils. It almost seems as if the strangler feels the powerful strain, as every now and again it draws another band round its victim. Sometimes the whole trunk becomes covered with a living pillar, only broken here and there by a few meshes, but more generally the bonds are not so complete. In the former case, death ensues very quickly; in the latter, the tree may linger for years. At times only a single limb is killed, after that a second, and so on, as its foe increases in strength. If the latter be perched on a branch overhanging a creek, the tree may escape without much harm, as most of the aerial roots will descend straight without touching the trunk.

After the tree becomes bare death soon ensues, and then it begins to break up. Twigs and branches fall by their own weight

or that of the epiphytes upon them. Then the bark begins to peel off and fall in great flakes which carry with them masses of orchids and bromelias. Beetles deposit their larvæ in the now rotting trunk, and wood-peckers make holes to get at them. Water trickles down into the hollows and makes them larger and larger until some day parrots build their nests there. Meanwhile, those destructive little creatures the wood-ants, or termites, are mining everywhere until the wood is permeated with tunnels only separated from each other by paper-like partitions. It takes many years before a large trunk is crumbled down under the influence of heavy rains and wood-ants, and during this time it runs the risk of falling, which means death to the strangler, since, deprived of its support, it cannot rise again.

On the sand-reefs which extend in front of the forests of Guiana, the struggle for life is less intense, as few trees can get over the difficulties attendant on a stretch of white sand heated by the burning sun. Seeds of trees that have been accustomed for generations to germinate in the damp gloom cannot endure this furnace-like heat. Clusias, on the contrary, are well fitted for such conditions, and grow to perfection on the reefs. Accustomed to throw their aerial roots downward, this faculty becomes very useful, as it enables them to reach the cool depths where moisture accumulates and is retained, even in the driest season. Among the half-starved looking bushes which grow in clumps here and there, the various species of the clusia family are conspicuous for their healthy appearance. Here, as elsewhere, they allow no epiphytes or parasites to rest on their smooth limbs.

Beside the stranglers there are those veritable blood-suckers, the many species of *Loranthus* and *Phoradendron*. These are allied to the English mistletoe, and like it parasitic. But the mistletoe works in a very quiet way as compared with its tropical cousins. Here in the cultivated districts of British Guiana thousands of fruit trees are infested with loranths, and many may be seen in the various stages from weakness to death.

Like those of the wild figs, the seeds of the parasites are distributed by birds. In wet weather they may be seen germinating on every fence—little discs with two mistletoe-like seed-leaves. The pulp round the seed is glutinous and keeps it from falling until the sucking disc, which answers to the root, takes a firm hold. Those that germinate elsewhere than on the living tree soon wither, but the others take firm hold and soon extend their aerial roots up and down the branch or twig. These are provided with suckers at intervals of about an inch, by means of which the juices of the tree or shrub are extracted until the loranth begins to rapidly spread in all directions. Its long whip-like branches wave in every breeze and are often carried near other trees or other boughs of that on which it is located, seizing them by its aerial roots which coil spirally round them for a foot or more, until they are tapped by a dozen suckers. In this

manner the octopus-like arms are extended everywhere, and the loranth grows into a great bush.

It is not, however, to the interest of the loranth that the tree should die, but rather live as long as possible. It therefore avoids the great limbs and larger branches, confining itself to the smaller twigs. Nevertheless, a branch is often killed by the continued depletion, and if the loranth has not succeeded in laying hold of others it soon withers. Sometimes every branch is infested and then the whole tree must succumb, to the utter destruction of its destroyer. As in the case of the strangler, flowers are at first produced without fruit, then the tree gets too weak to bloom, and finally leaf after leaf drops until all are gone, and the branches themselves die and fall off. When the tree is covered with foliage the loranth is hidden, but when it is bare the enemy can be seen in all its luxuriance. Where trees are close together, as in the forest, it rarely happens that one is actually killed in this way, as the loranths attach themselves to several individuals. Nevertheless, in such an intense struggle as that of the forest any element of weakness is a dangerous handicap to the individual, and will ultimately cause its defeat.

Where there are no deciduous trees, it follows that one with its twigs entirely bare is a very striking object. Everything around is most luxuriant, while the victim of the strangler or parasite stands like the mummy at a feast. This does not last long, however, as bush ropes soon cover it with garlands of flowers, and in a year or two young trees shut out the sight.

Palms and other endogens escape both clusia and loranth. Sometimes a fig will establish itself in the crown, and even encircle the stem, without the slightest injury to its host. The strangler may encircle and compress it, but the vital part is unaffected, being well in the interior and protected by very hard and tough surroundings. Moreover, the immense fronds or leaves are too hard and dry to be congenial to the loranth. Unfortunately, however, it is not entirely secure, although it folds frond after frond over the tender terminal bud. The borer beetle finds out its weak point, pierces all the wrappings, and deposits its eggs, with the result that the heart is soon devoured by larvæ. Then the fronds drop off and nothing remains but a bare stem like the pillar of some ruined temple lost in the forest.

Accidents cause the death of many a forest tree on the river bank. High floods soften the mud, so that when the trunk leans over and is weighted by a burthen of creepers, it is often borne down and even carried away by the current, if the timber is not too heavy to float. Sometimes, on a creek, the tree will be entangled in the branches of its opposite neighbour, and manage to recover itself by throwing out fresh roots. When the wood is heavier than water and the tree settles down and dies, its trunk remains as an obstruction for a very long time, perhaps centuries. When the creeks are very low, these old trunks or tacoobas are seen lying at the bottom in the utmost confusion. Nothing

remains but the heart-wood, which is very hard, and may last for an indefinite period at the bottom of the stream, as wood-ants and other destructive agencies are unable to finish the work of destruction under such conditions. Some of the tidal creeks are so choked with these tacoobas as to be impassable at low water, while in others great care is required. With a stream like a mill-race, the bateau runs a risk of being bored at any turning, while every few minutes it bumps and grazes the sunken logs. Sometimes large trees float down the river and are carried out to sea.

The forest trees of Guiana seem to have lost the power of shooting up afresh from the roots when injured. It is true, as mentioned above, that a tree rarely breaks off or is torn up by the roots until it is actually dead. Storms are rare, and tornadoes unknown, but it sometimes happens that one of the tallest is struck by lightning. Again, the fall of every rotting trunk kills or seriously injures everything that stands in its way. Great limbs are torn off, leaving unsightly gashes, which sometimes heal, but as often fester, ultimately producing holes which continually grow larger as water finds its way into them. When once the outer bark and sap-wood is seriously injured, beetle larvæ and wood-ants soon carry on their work of destruction, and death must ultimately ensue. At first sight it would appear as if when the tree is in the embrace of the strangler, or bleeding to death under the thousand suckers of the loranth, it would shoot up from below and try to recover its position, but such is not the case. The tree has had to fight its way upward for ages, and all its powers are concentrated for this purpose.

The natural vegetation of a country is well fitted to cope with all the difficulties of its environment. Parasites have been developing their powers for ages, but never succeed in making any serious inroad on the forest. As the enemy increases in strength and malignity, its victim gains more power of resistance. With plants from other countries, however, it is different. Taken from their particular environment to another, against dangers of which they have no protection, they suffer greatly. East Indian fruit trees, notably the Mango, are often diseased, while ornamental shrubs are as frequently infested by loranths. Rarely is a forest tree killed by umbrella-ants, but almost every imported plant is destroyed by them. Some districts in Guiana are so infested by these leaf-cutters, that cultivation is impossible. In early times we read of plantations being abandoned on that account. It was useless to attempt any cultivation, since myriads of these little creatures carried away the young leaves as fast as they grew. Butterfly larvæ are equally destructive to foreigners. In a day or two, a fiddle-wood tree (*Citharexylon*) will be stripped perfectly clean by swarms of these voracious creatures. Rarely does the tree recover from such an attack, although it sometimes succeeds in doing so. The sudden defoliation of a whole tree

is so severe a check to its growth, that even if it should overcome one such calamity, it is weakened for a long time afterwards.

Imported fruit trees, and especially palms, are much infested by scale insects. These cover the under surface of the leaves and suck the juices, causing sickness, and even death. Often they are accompanied by fungoid growths, which hasten the end. The mango is much infested by both scale insects and fungus, and may live in this condition for many years. The under surfaces become spangled with a pretty star-like scale, and as this carries on its work of depletion, the foliage begins to look dirty and unhealthy, while in its later stages the fungus flakes off. At first the tree loses its power of ripening fruit, then no flowers are produced, and finally it may linger for years a centre of infection to others, until the owner chops it down as an unsightly pest. It does its best to get rid of its parasite by dropping leaf after leaf, and developing new ones, but both scale and fungus multiply and attack them almost as soon as they unfold.

Unlike some other countries, Guiana has few herbivorous animals. Tapirs and manatees do not interfere with the forest trees, and even the rodents confine themselves to fruits or water plants. The young seedlings are never bitten off, nor is the bark gnawed. It follows, therefore, that protective measures against the larger animals are unnecessary, and every effort can be used for other purposes. Unhampered by outsiders, the plants only contend among themselves, thus contrasting with European vegetation, where thousands of signs indicate a struggle with outside enemies which in the tropical forest are almost entirely wanting.

JAMES RODWAY.

VII.

Some Recent Researches on the Movements of Diatoms.

THE announcement a short time ago of the discovery of supposed pseudopodial processes in Diatoms by Mr. J. G. Grenfell, in the *Quarterly Journal of Microscopical Science* (October, 1891) has excited much criticism from various sides, some of which may be found summed up in a short note on the subject in NATURAL SCIENCE for April, 1892 (p. 94). In the words of Mr. Jabez Hogg, quoted in that notice, the processes in question "are, in no sense of the word, pseudopodia, neither are they, nor can they be regarded as, organs of locomotion; and their discovery—which, by the way, is a very aged one—throws no light whatever on the debatable question: the movements of diatoms."

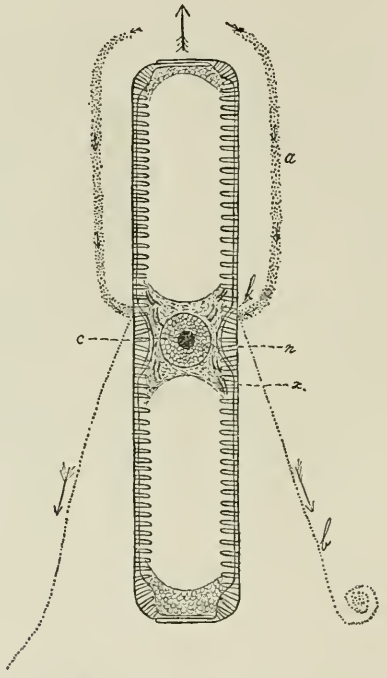
In March of this year a paper was read by Professor Bütschli, of Heidelberg, before the "Naturhistorisch-Medizinischer Verein" of that University, containing a preliminary account of some researches carried on by himself and a student, Herr Lauterborn, upon the movement of a large species of Diatom.[1] The results obtained are so novel and important that, from this reason, as well as from the fact that the journal in which they are published is one not easily accessible to English readers, no further excuse is needed for bringing them before the notice of the general scientific public.

The researches in question were conducted on a large species of *Pinnularia*, which Herr Lauterborn identified as *P. nobilis*. M. Schultze had already shown that in various Diatoms foreign bodies could often be seen gliding along the so-called *raphe* in the middle line of the side of the shell, and concluded from this and other observations that the shell valves could be opened for a longer or shorter distance along the course of the raphe, and that here the protoplasm, which plays a part in the movement, comes to the surface. This observation was frequently repeated, and finally the authors were able to obtain more exact results by placing specimens of *Pinnularia nobilis* into water containing Indian ink. By careful observation of the Diatom in side view, it could be seen that at the ends of the raphe nearest to the so-called *noduli* in the middle of

[1] Verhandl. Naturhist.-Med. Vereins zu Heidelberg, *n.s.*, vol. iv., p. 5.

each shell valve, little clumps of fine granules of Indian ink were formed, which increased gradually by the addition of more granules. After a time it could often be observed that a fine thread shot out from the heap of granules, which ran along the raphe, though not directly upon it, to one end of the diatom.

The investigation of a specimen lying upon the zonal side gave a clearer insight into the real course of the phenomenon (see Figure). From in front of the diatom a stream (*a*) of loose granules, each separately suspended in the liquid, flows along each side in a backward direction towards the nodal point of the raphe (*k*). At this spot



Optical section of a *Pinnularia nobilis* in movement, in zonal view. *n*, Nucleus; *c*, the Centrosome; *x*, the peculiar double threads in the protoplasm; *a*, the granules flowing back to the nodal point (*k*) of the anterior raphe; *b*, the jelly thread shooting out backwards, which has rolled itself up into a tangle on the right side. The arrows show the direction of the movement of the Diatom, the granules, and the thread.

All the details drawn in the figure can be plainly observed in the living Diatom.

they become fastened together by an invisible connecting medium, and then continue to travel backwards united into a thread (*b*), which passes obliquely outwards and backwards, becoming thus further and further removed from the surface of the shell-valves. As the thread is formed the diatom moves forward in the direction of the arrow. It is evident that here we have to do with a thread which is simply made visible by the sticking together of numerous Indian ink granules. This can be recognised (1) by the regular arrangement of the granules in rows, (2) by the cessation of the usually so lively

molecular movements as soon as the granules are united with the thread, and (3) from the circumstance that the end of the thread sometimes winds itself up into a tangle. Moreover, the thread not unfrequently appears interrupted for a stretch, owing to the Indian ink granules being wanting for some cause on this portion of it, it may be on account of less adhesiveness of the thread, or from want of granules at the moment of its formation. Usually the thread is long enough to project back beyond the diatom ; at times, however, the formation was observed of unusually long threads, far exceeding the whole length of the diatom. But after a time the threads break off, together with the clumps formed by rolling up of their ends, and then they remain still while the diatom continues to move forwards. The formation of the thread proceeds by jerks, which harmonises with the peculiar jerking movements of the diatom.

Sometimes, instead of a thread on each side running backwards, as when the diatom is gliding forwards, one thread runs backwards, the other forwards, and the diatom revolves. In some instances it was possible to follow the behaviour of the threads during a reversal of movement. The threads carried at their extremities a clump of ink granules. After the diatom had come to rest, it could be observed that the clump was drawn back to the nodal point of the raphe, apparently by shortening of the thread. In a short time a new thread was developed, pushing out the clump of granules at its extremity, but in the opposite direction to the former thread. The diatom then began to move in a direction opposite to that it formerly took.

From these observations Professor Bütschli draws the following conclusions :—" It can hardly be doubted that the jerking elongation of the threads stands in causal connection with the locomotion of the diatom. Now, since it is known that diatoms, as a rule, only move upon a solid surface, it would be an obvious assumption that the threads fix themselves in some way upon the surface, and by their elongation push the diatom forwards in jerks. On the other hand, however, the rocket-like shooting out of the threads would be quite sufficient, even without fixation of their ends, to explain, by means of the resistance of the water, the jerking progress forwards of the diatom. To me, indeed, the latter explanation appears to have more in its favour, since we never could determine with certainty any fixation of the threads to the underlying surface ; and the above described tangle of their ends, and, further, the pulling back and apparent drawing in of the thread during change in the direction of the movement, speak against such a fixation. There would always be a possibility that the peculiarity of our fresh-water diatoms, of moving only upon a solid surface, is connected with the fact that they cannot as a rule float. We should then have to refer the cause of diatom movement to an abundant production of sticky jelly, which shoots out quickly and with some force at the nodal points of the

raphe in the form of fine threads. Then the movement of diatoms would have a great resemblance to that of Desmidiaceæ, which, according to the investigations of Klebs, similarly move with the help of secreted threads of mucus."

The threads would be completely invisible, owing to their transparency and similar refrangibility to the surrounding water, if it were not for the peculiarity of ink granules sticking to them, and it was not found possible to colour them with methyl blue, Congo red, or other aniline stains. Sometimes, however, the adhesiveness appears to be wanting, since frequently nothing could be seen of the threads in actively motile specimens, and the same was the case in a series of other diatoms examined, *e.g.*, various *Navicula* species, &c. Perhaps, in these cases, the secreted jelly dissolves quickly again, and the formation of the threads observed in *Pinnularia* is suppressed.

The invisibility of the jelly, without application of special methods, is further shown by the fact that, in the investigation in Indian ink of *P. nobilis* and other species of the genus, it could be demonstrated that these forms are, as a rule, covered with a more or less considerable coat of jelly. It is only recognisable by the fact that the Indian ink does not penetrate the region of the jelly envelope, and even with the application of this method no sharp boundaries to the envelope could be determined with the strongest magnifications. In the motile specimens it could at least be seen that the jelly was interrupted at the nodes on the sides of the shell, so that no hindrance was opposed to the exit of the thread. The anterior current of ink granules takes its course to the nodal point of the raphe along the outside of the jelly envelope, as a result of which it always remains at a definite distance from the surface, as shown in the figure, and first sinks into the raphe at the nodal points. From the presence of this envelope the question might arise whether the supposed jelly thread is not really a thread-like tract of jelly which passes along the surface of the shell, and first becomes free posteriorly, but the authors believe this is not the case.

Finally, the authors promise us a continuation of these observations, to which all interested in microscopic animal and plant life will eagerly look forward.

In these researches we have an entirely new light thrown upon what has always been so great a puzzle, the movements of Diatoms, and it is obvious that any form of pseudopodia is quite unnecessary and inadequate to explain it, thus quite justifying the words of Mr. Jabez Hogg quoted at the commencement of this article.

E. A. MINCHIN.

SOME NEW BOOKS.

Darwin and after Darwin. An Exposition of the Darwinian Theory and a Discussion of Post-Darwinian Questions. By George John Romanes, M.A., LL.D., F.R.S. I.—The Darwinian Theory. 8vo. Pp. 460. London: Longmans, Green & Co., 1892.

The undertaking originally planned by Mr. Romanes was sufficiently extensive. It was nothing less than a complete survey of the doctrine of organic evolution from its beginning in the earliest recorded times down to the present day. He intended to elaborate and expand the material of the course of lectures delivered by him in the Royal Institution in the years 1888 to 1890 into a systematic treatise in three separate volumes. The first part, dealing with pre-Darwinian evolution, is indefinitely postponed, wherefore the title "Darwin and after Darwin" has been substituted for "Before and after Darwin," the title of the series of lectures. The second part is contained in the volume before us, and is confined to the Darwinian theory, while the third part will follow soon under the sub-title Post-Darwinian Questions.

The author, in his preface, proceeds to tell us that the present volume is thus intended to be merely a systematic exposition of what may be termed the Darwinism of Darwin, and that, as on this account it is likely to prove of more service to general readers than to professed naturalists, he has been careful to avoid assuming even the most elementary knowledge of Natural Science on the part of those to whom the exposition is addressed. One would suppose that the Darwinism of Darwin meant Darwin's own argument and his own illustrations, and would prepare to judge the book according to the soundness of the exposition and the force and lucidity with which it appeals to the average intelligence of those who have been unable to study, or unable to understand Darwin's own writings. But on reading the book itself we find that though it may be said to expound the Darwinism of Mr. Romanes, there is much in it which is not derived from Darwin, while much that is most characteristic of Darwin is only conspicuous by its absence.

In fact, in the introduction a different description is given of the purpose and character of the book. The author here states that he wishes to present the whole theory of organic evolution as he believes that it will eventually stand; that his endeavour is to exhibit the general structure of the theory in what he takes to be its strictly logical form, rather than to encumber any of its parts by a lengthy citation of facts. The present volume is concerned with the theory from the appearance of the Origin of Species in 1859 to the death of its author in 1882. It is to be a condensed and critical statement of the main evidences and the main objections which have thus far been published with reference to the distinctively Darwinian theory, the chief novelty being the pictorial illustration of the facts on which the arguments are based.

The book, however, does not correspond to this description much more exactly than to the other. Section I., which is more than half the volume, is devoted to what the author calls "the main evidences of organic evolution considered as a fact." The theory of organic evolution is not the distinctively Darwinian theory.

The view expressed at the commencement of the "Origin" is that, however cogent the evidence that organic forms have been evolved by descent, the demonstration of that truth would be unsatisfactory without an explanation of the manner in which adaptations and perfection of structure had been acquired. Accordingly, nowhere throughout the "Origin of Species" is the evidence for the occurrence of evolution considered apart from the evidence for Natural Selection; and the same thing is true in Darwin's other books. There were evolutionists before Darwin who knew nothing of Natural Selection, and many of his adherents and disciples have shown more interest in the course and the details of evolution than in its causes. The most prominent of these was Haeckel, and the first section of Mr. Romanes' book may with greater truth be described as Haeckelism than as Darwinism. The distinction between the occurrence of evolution and its explanation, is, of course, a most important one, but no one has pointed out more clearly and emphatically than Haeckel that Darwin's title to glory rests on his contributions to the latter, and not on his discovery or establishment of the former. It is astonishing that Mr. Romanes should have drawn so largely from Haeckel's writings, and in particular from his "Schöpfungsgeschichte," and yet have ignored the truth of the following paragraph which occurs in that work:—"Darwin's merit is over-estimated when he is regarded as the founder of the Theory of Descent, or of the whole of the Theory of Development. We have seen from the historical sketch in this and the preceding chapters, that the Theory of Development, as such, is not new; all philosophers who have refused to be led captive by the blind dogma of a supernatural creation have been compelled to assume a natural development. But the Theory of Descent, constituting the specially biological part of the universal Theory of Development, had already been so clearly expressed by Lamarck, and carried out so fully by him to its most important consequences, that we must honour him as the real founder of it. Hence it is only the Theory of Selection, and not that of Descent, which may be called Darwinism."

In expounding the evidence on which the Theory of Descent is based, Mr. Romanes, like Haeckel, also endeavours to disprove the theory of Special Creation. This was natural enough in Haeckel's work, which included a survey of the history of the subject; but Mr. Romanes makes no attempt at historical treatment. He contrasts the logical consequences of the two theories with one another, and with the facts of organic nature; but it is doubtful whether the proof of the evolution theory is in any degree made logically stronger in this way. My own opinion is that it is not. As the creation theory has no strictly logical basis, so it has no strictly logical consequences. Let us take, for instance, one example of Mr. Romanes' method—his discussion of the controversy between Mivart and Darwin concerning the eye of the octopus. He says, when it is proved that the eye of the cephalopod is of quite a different plan of structure from that of the vertebrate, the special creationist can only reply that it may have pleased the Deity to form a certain number of ideal types and never to allow the structures occurring in one type to appear in any of the others. He argues, then, that in that case we should

expect the same typical structures to be always present within the limits of the same type; and, as this is not the case, the hind limbs being absent, for instance, in the whales, the assumption of the creationist must be abandoned. But this is obviously a *non sequitur*. It would be equally true to say that the absence of hind limbs in the whale proves that the animal does not belong to the vertebrate phylum.

Even if we consider the facts of embryology, which are the most striking evidence of the truth of evolution, it is not possible to show that they are absolutely incompatible with the creation doctrine. The creationist may say, if he pleases, that when man was created it was ordained that he should pass through fish-like stages in his embryological development, so that the ideal unity of the vertebrate phylum might be more perfectly maintained. It is impossible to disprove such an assumption. The logical reply to the creation hypothesis is that it assumes a multitude of miraculous occurrences for which there is absolutely no room in Nature, so far as we know it by human observation and reasoning; while at the same time the hypothesis as upheld by naturalists in Darwin's time, including a succession of creative epochs, could lay no claim to support from supernatural revelation.

Mr. Romanes' restatement of the argument for evolution is divided into five chapters, containing the evidences from Classification, Morphology, Embryology, Palæontology and Geographical Distribution respectively. Of these, the chapter on Embryology is by far the worst, being unfortunately crowded with errors and inaccuracies. The first case described of the persistence of ancestral organs in the embryo, is that of the external gills of the fœtus of the Alpine Salamander. This case is mentioned by Darwin on the somewhat unsafe authority of G. H. Lewes, who wrote that the gills of the fœtus had no reference to the future life of the animal, nor any adaptation to its embryonic condition, that they had sole reference to ancestral adaptations, and repeated a phase in the development of its progenitors. Mr. Romanes still supposes that this is a correct account of the matter, although it must be evident to anyone who has studied Fraülein von Chauvin's paper on the subject that the fœtal branchiæ have been specialised and enlarged for the purpose of intra-uterine respiration. It is true that the fœtal branchiæ are capable of aquatic respiration outside the body of the mother, but it is also true, although not mentioned by Mr. Romanes, that the great size and delicacy of these branchiæ were the actual cause of death to all the larvæ with which Fraülein von Chauvin experimented except one, and that one escaped the fate of its brethren only by getting rid of its uterine gills and growing new ones of more serviceable size and character.

A large number of pages are next devoted to a detailed discussion of the relations of the Metazoa to the Protozoa, a discussion which it is very difficult to criticise. It is certainly interesting, but it also certainly belongs in many respects to the post-Darwinian, not the Darwinian period. The discussion is difficult to follow, and I fear will not be thoroughly intelligible to the general reader who is destitute of even the most elementary knowledge of Natural Science, for it requires some acquaintance with Weismann's theories to understand the author's meaning. He says, for instance, that not only the individual development but also the powers of asexual reproduction on the part of multicellular organisms are all ultimately due to the specialised character of their germ-cells. He points to the karyokinesis of the segmenting ovum and the formation of the polar

bodies as evidence of this specialised character, and then proceeds to consider to what extent these peculiar complicated phenomena are represented in the Protozoa. Of the expulsion of polar bodies, he says that nothing resembling it has ever been observed in any of the Protozoa—an unjustifiably dogmatic statement, inasmuch as portions of the nucleus have been observed to be ejected in the conjugation of the Ciliata. One of the general conclusions drawn from the discussion is that no line of real demarcation can be drawn between growth and reproduction, even of the sexual kind. But the course of the argument has by no means succeeded in filling up the chasm of difference between the division of one cell into two, which is growth, and the union of two cells into one, which is sexual reproduction.

The rest of the chapter is devoted to the embryogeny of the Metazoa beginning from the fertilisation of the ovum. In the description of the characters of ova an unfortunate error is made, the pores of the zona radiata in the Mammalian ovum being referred to the same category as the micropyles of other ova.

The majority of the instances of embryological correspondence described are derived from Haeckel's more imaginative writings, but Mr. Romanes is placidly unconscious that doubt has been thrown on the objective reality of any of them. He does not even quote Haeckel's fascinating, though sometimes unfortunate, speculations with strict accuracy. On one page he figures the gastrula of the zoophyte *Gastrophysema*, and on another gives two figures of *Prophysema primordiale*, an extant gastrula-form, all "after Haeckel." As far as I have been able to discover, Haeckel never used the name *Prophysema* at all. The figures described under this name are copied from those of *Halophysema primordiale* in Haeckel's paper "Die Physemarien," and were proved 12 years ago to represent an organism specially created by Haeckel himself, *Halophysema* being really a reticularian Protozoan. *Gastrophysema* was another genus of Haeckel's imaginary extant Gastræa, and I have been unable to discover where among Haeckel's works Mr. Romanes found a reference to the gastrula of a zoophyte called by the same name.

It is stated that probably all the Metazoa pass through the gastrula-stage, which is correct, if by the latter term is merely meant the diploblastic condition. But only one process by which this condition is reached, namely, invagination, is described, and the gastrula of Olynthus, "after Haeckel," is figured without any mention of the fact that it is not produced by invagination. Another figure is described as the "gastrula of an Arthropod (*Nauplius*)," as though *Nauplius* were a genus.

Still more extraordinary, however, is the foot-note on p. 139, which states that in most vertebrated animals the process of gastrulation has been more or less superseded by another process called delamination, but that even in the higher Vertebrata embryologists are pretty well agreed as to delamination having been merely a later development of, or possibly an improvement upon, gastrulation. Perhaps the author has confused Professor Lankester's theory that gastrulation is originally derived from the delamination observed in some Coelenterata, with some faint reminiscence of the modified epibolic gastrulation of fishes and birds. The rest of the chapter is still more strongly permeated with Haeckelism. We have the figure of Haeckel's ideal primitive vertebrate, and two pages of the well-known somewhat ideal figures of embryos of fish, pig, man, etc. On p. 147 we note the peculiar statement that the gill-slits are supported

internally by gill arches, or the blood vessels which convey the blood to be oxygenised in the branchial apparatus. It is really a pity that the author was not more careful to prevent misconception on the part of the general readers for whom his book is intended. Mr. Romanes claims to belong to a class of naturalists whom he calls specialists in Darwinism, and says that the opinion of those who have done good work in other departments may be destitute of value in questions of evolution. However this may be, it is certain that the specialist in Darwinism who undertakes to expound the embryological argument for evolution should not rely almost exclusively on the writings of Haeckel. He would find Balfour's "Comparative Embryology" a safer guide. Mr. Romanes would have been wise to submit this unfortunate chapter to the revision, if not of a specialist in embryology, at any rate of some zoologist with a competent knowledge of that department, for instance to that of his friend Professor Lankester, whom he quotes with great admiration.

The second more truly Darwinian section of the book is much more satisfactory than the first. The theory of Natural Selection is clearly and fairly stated. It is pointed out that in its main elements the theory is merely a statement of observable facts, the fact of the excessive rate of reproduction leading to the constant pressure of numbers in each species, the fact of competition, or struggle for existence and consequent survival, the facts of hereditary transmission and individual variation. We welcome the candid admission that the theory only explains changes in organisms so far as these changes are of use; in other words, that it is a theory of the origin of adaptations, not of species. This admission occurs in an interesting discussion of misconceptions of the Darwinian theory which are current, not among its opponents, but among its supporters. Another of these misconceptions is that the theory can explain all cases of modification, whereas in some cases it is not logically possible that it can apply; others are that it follows deductively from the theory itself that Natural Selection must be the sole means of adaptive modification, and that all hereditary characters are necessarily due to Natural Selection.

In the chapter on the evidences of the Theory of Natural Selection we have three general or main arguments:—The observed fact of the extermination of forms in the struggle for existence; the consideration that we cannot find an instance of a structure or instinct developed for the exclusive benefit of another species; and, thirdly, the facts of domestication. This last class of facts is illustrated by fourteen pages of figures drawn from actual specimens of domestic breeds, illustrations which form the most novel and distinctive feature of the book. These figures, although not very beautiful, are certainly of great practical use, and enable one to appreciate the peculiarities of the several races better than the most elaborate description without figures. As far as we can judge, they are not only vigorous, but accurate.

Then follows a discussion of some of the detailed applications of the theory to the explanation of adaptations, the author having decided to select all his instances from a single class, namely, that which may be generically termed defensive colouring. This choice is, it seems to me, extremely unfortunate at a time when so many popular treatises have just been produced on the same particular subject. As we have so recently had to study Mr. Poulton's discussion of the subject on one side, and Mr. Beddard's on the other, it would have been a relief to find in Mr. Romanes' book the exposition

of some other class of adaptations. Mr. Romanes has little that is new or original to say on this familiar subject, and we find the well-known cases treated in the same familiar way. The resemblance of the butterfly *Kallima* to a leaf is illustrated once more, here in an uncoloured figure, which contrasts to great disadvantage with the splendid chromo-lithograph in Mr. Beddard's book. As usual, no information is given concerning the actual habits of *Kallima* in its natural environment, the particular leaves which it resembles, or the enemies which it deceives. The last sensational discovery by Mr. Sclater, described by Mr. Poulton, of a homopterous insect whose body mimics a leaf-cutting ant, together with the leaf it carries, is here reproduced; but, as in the original description, no attempt is made to show that the mimicry is of any advantage to the insect which exhibits it. Another case given is the imitation of a venomous snake by a non-venomous one, and considering the difficulty, even to a naturalist, of distinguishing at first sight the innocence or otherwise of any snake met with in its native home, it would have been by no means superfluous if this case had been clearly proved to be one of true mimicry.

Chapter IX. is devoted to criticisms which have been put forth of the theory of Natural Selection, and to some at any rate the most interesting part of this chapter is that which deals with the electric organ of skates and rays. Here the author, after giving an interesting and detailed account of the structure and relations of these organs, and discussing very impartially how its evolution can be explained on the theory of Natural Selection, confesses with a candour which does him the greatest credit that in the present state of our knowledge such an explanation is quite impossible. He even goes so far as to say that if a number of such cases could be produced, the theory of Natural Selection would have to be discarded.

The last chapter deals with the theory of Sexual Selection, which the author warmly defends against Mr. Wallace's criticisms. Mr. Wallace's chief objection is that if the secondary sexual characters supposed to be due to Sexual Selection are not necessarily correlated with that fitness which nature selects, then the fit that do not possess them would be selected, while if they are so correlated then it is nature which really selects them and not the opposite sex. Mr. Romanes states what in his judgment would have been Mr. Darwin's reply, and in so doing presumably gives his own. It amounts essentially to this: that secondary sexual characters are of too definite and elaborate a kind to be regarded as the mere concomitant of that exuberant health and vigour which are the result of Natural Selection. It may be admitted, says the author, that a general brilliancy of colour might accompany a general increase of vigour, but it does not follow that the particular disposition of colours in the form of ornamental patterns, and also elaborate special structures, can thus be accounted for by Natural Selection. In this contention we entirely agree. But Mr. Romanes is by no means successful in avoiding the other horn of the dilemma. He merely says that "in the phenomena of decorative colouring (as distinguished from merely brilliant colouring), of melodious song (as distinguished from merely tuneless cries), of enormous arborescent antlers (as distinguished from merely offensive weapons), we have phenomena which cannot possibly be explained by the theory of Natural Selection; and, further, that if they are to be explained at all, this can only be done, so far as we can at present see, by Mr. Darwin's supplementary theory of Sexual Selection." It seems to us that a much more

forcible answer than this can be made, to wit the following: Mr. Wallace has been to some extent misled by the influence of language, Natural Selection and Sexual Selection being unconsciously conceived by him as independent powers or causes. He has forgotten that, however successful an individual male may be in the struggle for existence, he cannot bequeath his superior endowments and faculties to offspring unless some female appreciates him sufficiently to grant him the favours of sexual intercourse. Mr. Wallace argues as though the superior male, *facile princeps* in the competition for a living, could found a line of descendants inheriting his own health and vigour without female assistance, and could afford to scorn and ignore the petty female standards. On the contrary, the fact is that there is competition in love as well as in life. It is an observed fact that males, in very many instances, fight together for the possession of the females, and, in other cases, compete with one another by the display of adornment or the efforts of song for the favour of the opposite sex. It is also an observed fact, in many cases, that where this sexual competition is most conspicuous there the secondary sexual characters are most developed, and that competition is conducted solely in respect of these characters. A male that excels in the struggle for existence is a complete failure, so far as the species is concerned, unless he can succeed also in finding mates. In view of the fact that competition for mates occurs among those males which have already survived the process of Natural Selection, and are all able and eager to beget offspring, it is a certainty that those will leave most offspring to succeed them who are most able, either by force or charm, by weapons or adornments, as the case may be, to constitute themselves the fathers of the next generation. Without going so far as Mr. Romanes, and saying that this is the argument that Darwin would have used, I venture to think it is more consistent with the Darwinian method and doctrine than that employed by Mr. Romanes himself. At the same time, the formulation of the argument is not intended to imply any admission that we regard the actual selection of individuals as the essential and most important result in sexual any more than in "natural" or general competition.

Subsidiary objections of Mr. Wallace's are, that there is no evidence that hen birds, for instance, are charmed by the beauty or voice of the males, and that, if there were, it would be necessary to the theory that the taste of the females should be uniform in all individuals and constant throughout many generations. Mr. Romanes' reply is practically what he condemns in another place as the argument from ignorance. We do not know, he says, what sentiments may be in the mind of a hen, and as for the constancy and uniformity, we know very little about the psychology of the lower animals. Here, again, I would venture to suggest a better reply. We do not require to assume an æsthetic sense in the hen birds. Without going deeply into psychology it is obviously probable that it is simply the sexual desire which is alone concerned in Sexual Selection. This desire is usually not easily excited in the female, and the function of subsidiary sexual organs and their display or exertion is probably enough the excitement of this desire, without which that of the male cannot be gratified. And the explanation on this view of the constancy of the selection in the same species is merely heredity. The sexual desire of the female has a hereditary association with certain sensory stimuli, and the means of furnishing these stimuli are constantly reproduced and improved by inheritance in the males. I will not develop these considerations further. They are, perhaps, too much beyond the

scope of the distinctively Darwinian theory to be relevant to Mr. Romanes' book.

The volume concludes with an appendix and notes, of which the former is a criticism of objections to evolution on palæontological grounds.

After noticing the salient features of the book at such length, it is unnecessary to add much concerning it as a whole. I will, therefore, simply finish with the opinion that while it is scarcely to be recommended to the unscientific reader as an introduction to the general philosophy of biology, it contains, notwithstanding its defects, much that is interesting and suggestive to the biologist.

J. T. CUNNINGHAM.

AN EXPLORATION OF DARTMOOR AND ITS ANTIQUITIES. By John Lloyd W. Page. Third Edition. Pp. xvii. and 316. With Map and Illustrations. London: Seeley and Co., 1892. Price 7s. 6d.

CONSIDERING its proximity to one of the most tourist-haunted areas in England, and the ease of access to it both by road and rail, it is surprising that Dartmoor remains one of the least-known of British moorlands. Doubtless this is largely due to the villainous reputation of its weather, originally established by the rhymes of local poets and the lamentations of French prisoners, and still maintained by the Princetown rain-gauge. This, however, cannot be a sufficient explanation while the Lake District continues to be visited by hordes of rain-soaked tourists, and the lack of popular literature has probably more to answer for. While the geology of Wales, the scenery of the Yorkshire moors, the literary associations of the Lakes, and the antiquities of the Wiltshire Downs have received constant description and discussion, the bibliography of Dartmoor has remained comparatively limited. Since Rowe's "Perambulation" in 1848, there has been no general work of merit on the area, except the admirable article by R. J. King in Murray's Guide, or the same author's "Dartmoor Forest and its Borders," until the publication of the present work; the issue of three editions in two years shows that it was needed.

The chief merit of the work is one which is its own reward, being sure to bring success; it consists not only in the author's knowledge of Dartmoor, but in his love for it. He is full of the stories and traditions of the moors, and of the legends of the demons that dwell among its granite tors, or of the pixies that skip about the boggiest of its bogs; he knows well the peculiar manners and customs that till recently survived in the seclusion of its dales; and he goes into raptures over the scenery, alike of the bare rounded hills of the central moorland, of the weird rocks along its margin, and of the picturesque valleys on its outskirts. His genuine enthusiasm for his subject in all its moods and aspects is contagious, and does much to disarm criticism.

The book is divided into five sections, one dealing with the general features of the moor, and the others treating particularly of the north, south, east, and west divisions. The especial sections are obviously based in the main on the author's journal, as he takes us along the lines of his different visits, pleasantly telling us of what he saw. The directions given, however, are quite insufficient for anyone to find the way by them, and the book thus combines the disadvantages of a topographical instead of a subject classification, without

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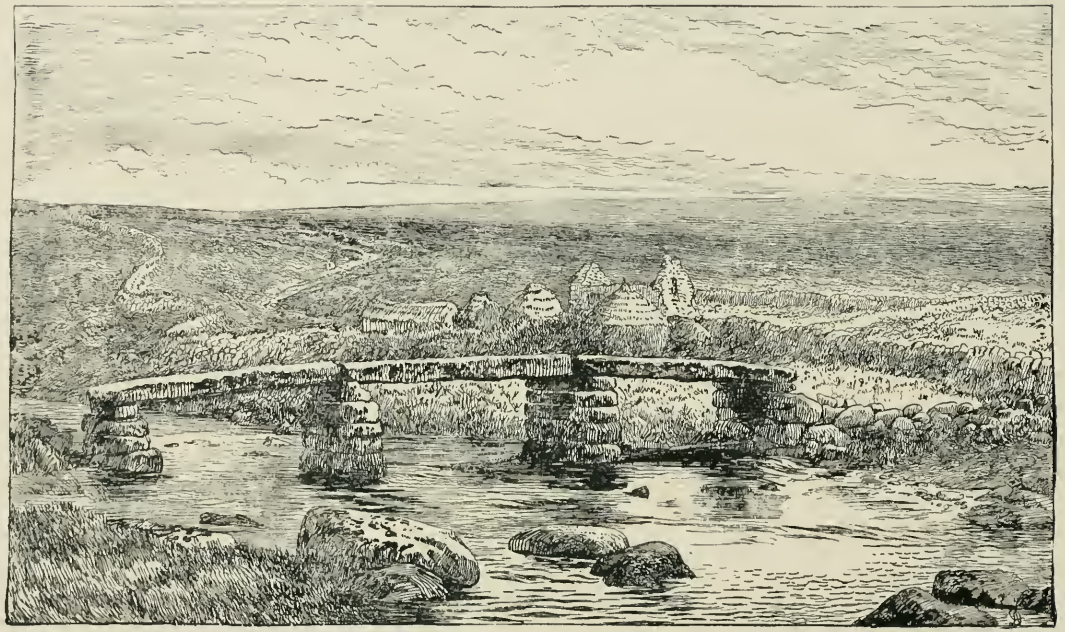

ANCIENT BRIDGE, POSTBRIDGE, DARTMOOR.

Drawn by ALFRED DAWSON.

its advantages. A guide-book account was not so much wanted, as we have those of King and Baddeley, and the work would probably have been more useful if it had followed the lines of a general treatment of the moor and its antiquities. It must, however, be admitted that the general and critical parts of Mr. Page's work are the least satisfactory. Let us, for example, take his remarks on the geology. His fond and frequent use of the word "trap" is symptomatic. We are not, therefore, so surprised when we read, on p. 13, that the granite is often porphyritic "from the presence of felspar." He quotes Mr. Ussher's hopeless theory that Dartmoor is a laccolite, without telling us that its author has abandoned that view. Another point which shows that the author has not fully grasped the significance of recent results, is that he quotes the theory that the schorlaceous and porphyritic granites and the elvans form a threefold series, without any hint that this has been rendered quite untenable by the acceptance of schorl as a decomposition product. We much wish that on p. 14 he would have given us some definite clue as to the exact locality "where no distinction could be drawn between granite and ordinary slaty material"; we always thought that the most famous feature of the contact-metamorphism around Dartmoor was the comparatively slight alteration the neighbouring rocks had undergone.

The Archæology is, as a rule, more reliable; the author's views are generally marked by common sense, except when, as on pp. 136, 137, he treats Ferguson's notions seriously. He is profoundly agnostic on all archæological problems, which is no doubt in the main justified by our ignorance of the facts that ought to be determinable. A comparison of Mr. Page's crude maps of the Merivale antiquities—great advance though they be on those of Sir Gardner Wilkinson—with such surveys as Petrie's of Stonehenge, shows how far our knowledge of these lags behind that of the Wiltshire monuments. Nevertheless, the author entertains little doubt of the pre-Roman age of the stone avenues and circles, the hut circles, the trilithons and the "clapper" bridges, the last of which are the chief of the unique features of Dartmoor. The bronze weapons and amber ornaments of the few barrows that have been well opened remove all doubt as to the occupation of the area in the Bronze age, and of the stream washing for tin that must then have been carried on. The general resemblance of the stone circles to those of Stonehenge, Karnac, and North Africa serve also to demonstrate that these are of approximately the same age as the Wiltshire series, which certainly date from the Bronze age. It does, however, seem difficult at first to believe that the clapper bridges have the same enormous antiquity; but their foundations are, as a rule, more substantial than those of the Stonehenge trilithons, and as most of these have survived, there seems no reason why the bridges should not also have done so. The rudeness of workmanship, the massiveness of the stones used, and the association of the bridges with the ancient British trackway, and groups of hut circles, all agree with the early age that is generally assigned to the bridges. The accompanying view of a typical example at Postbridge, which we owe to the courtesy of the publishers, is a good specimen of Mr. Alfred Dawson's beautiful drawings that adorn the volume.

In regard to the rock basins, we are glad to find Mr. Page admitting that these owe their existence to natural causes, dismissing the view of their artificial origin, which has been so strongly urged by Sir Gardner Wilkinson. The argument of this author—that they

must be artificial because they occur on other kinds of rock beside granite—would be all very well if he could find them on slate or limestone; but as he can elsewhere only quote them from the coarse grits of the Peak and similar rocks, which weather in exactly the same way as granite, they really support the view of their natural origin. The same explanation no doubt applies to the "ringed stones," for which the author adopts the term "tolmens"; and so many authors accept this word as synonymous with "dolmens," as does the Ordnance Survey in its map of Dartmoor, that a few words of explanation might well have been inserted.

The easy demolition of the "Druids' basins" encourages Mr. Page to run a tilt against the whole order of those holy men: he repeatedly denies all evidence for the existence of Druids anywhere on Dartmoor. If the author merely wishes to throw discredit on the fables of the burning wicker cages, which were probably invented for the edification of the ladies in Rome, and were no more reliable than the missionary stories of a later age, no one is likely to quarrel with him; but he seems to mean more than this. It is quite clear that the Bronze age people had a firm belief in immortality and some sort of religious ritual; this inevitably meant priests and collections. So long as one calls these priests "Druids" there seems to be little objection to the use of the term. To be consistent, Mr. Page must also deny the existence of Druids in Wiltshire, for the Drewsteignton Spinster's Stone is as fine a trilithon as the Fyfield Devil's Den; in that case he might as well deny the existence of Druids anywhere and invent a new name for the ancient British priests.

In spite of a constant tendency to feel dissatisfied with our author's treatment of the scientific part of his subject, we must congratulate him on having produced by far the best general sketch of Dartmoor. The book is well printed, and is illustrated by a series of etchings and photographic reproductions, several of which are the work of Mr. Alfred Dawson. The map, however, is very inadequate, and is of interest mainly as a sad example of amateur map-making. This will, we hope, be improved in the next edition, which should also be more carefully revised than the present. A very sanguine man might have been excused, in 1889, for saying that the prospects of the Postbridge Mine were promising: Mr. Page was certainly not a shareholder or he would have followed its later history more closely: nor is the Vitifer tin mine still working, as he tells us it is on p. 26. The work of the Devonshire Barrow Committee is also not mentioned. There is, in fact, little evidence in the book of any close acquaintance with the work of the younger school of archæologists, whose methods have not been as yet much adopted on Dartmoor. So far the excavations there have been conducted in a most casual manner, and not until the theodolite, chain, and spade have been applied with modern precision and thoroughness can we hope to make the stone avenues, rock circles, clapper bridges, and trilithons of Dartmoor yield their contributions to the prehistoric history of our land.

The Anatomy, Physiology, Morphology, and Development of the Blow-Fly (*Calliphora erythro-cephala*): A Study in the Comparative Anatomy and Morphology of Insects. By B. Thompson Lowne, F.R.C.S., F.L.S. Vol. i., pp. x. and 350. With 21 plates and 47 figures in the text. London: R. H. Porter (for the author), 1890–2. Price £1 10s.

In 1870 Mr. Lowne published a small book on the Anatomy and Physiology of the Blow-fly. The present volume is by no means a

second edition, but an entirely new work, as is necessary from the great advances in our knowledge of the structure and development of insects during the last twenty years. This first volume deals with the anatomy of the larva, the development of the embryo and of the imago, the external anatomy of the imago, and its histology. There are also chapters on the general life-history of the blow-fly, the general anatomy and morphology of insects, and the characters of the Diptera. The volume has appeared in three parts, the first issued in October, 1890, the third in April, 1892. In the second volume, which has yet to appear, we are promised "a detailed description of the various internal organs, their development, and physiology."

The study of insect anatomy has been neglected in this country, and the later literature of the subject has mostly come from the laboratories of Germany and Russia. We have, however, Miall and Denny's excellent monograph of the Cockroach, one of the most generalised of insects. Mr. Lowne's work is much larger than this, his type being a highly specialised insect.

It is impossible not to admire the thoroughness with which Mr. Lowne has carried out his task, and the laboriousness of his investigations deserves every acknowledgment. The plates representing the parts of the exoskeleton of the thorax are striking examples of this, no less than fifty-three sclerites being figured. In his description of the structure of the feet, he confirms the oldest suggestion as to the method of walking on plane surfaces: that the hairs on the pads secrete drops of viscid fluid which temporarily attach them to the surface, instead of the later theories that the hairs act as hooks or the pads as suckers. The mechanism at the base of the wing consists of several sclerites, and their action in producing the motion of the wing-tip is described. The curve described by the tip is of complicated form, and closely agrees with that arrived at for dragon-flies by Lendenfeld by means of instantaneous photographs.

In his interpretation of the proboscis, Mr. Lowne differs from most insect-anatomists. This organ in the fly has generally been regarded as almost entirely formed by the second pair of maxillæ (labium or lower lip) as it undoubtedly is in the Hymenoptera. Mr. Lowne, however, considers that he has evidence, from its structure and development in the blow-fly, that it really represents the first pair of maxillæ, and that we must consequently compare it with the sucker of a moth, not with that of a bee. He would derive the rostrum of the Hemiptera from the same source. The labium of the fly he supposes to be quite rudimentary. The piercing stylets which are found in many Diptera, and have always been regarded as the mandibles and the first pair of maxillæ, he considers, as well as the sucker, to represent parts of the first pair of maxillæ only. The similar stylets in the Hemiptera would probably be explained in the same way. These views will certainly not be accepted without further evidence, but they deserve careful consideration, and will stimulate research.

An embryological point on which Mr. Lowne is at variance with most other observers is the nature of an invagination in the dorsal region of the blastoderm (dorsal organ of Kowalevsky). This structure, regarded by Graber as the proctodæum, is thought by Mr. Lowne to be an archenteron, and, therefore, to show a typical gastrulation. The Malpighian tubes are developed as sacculi from this cavity; hence if it is an archenteron, they are coelomic in origin, and show more affinity with the excretory organs of other invertebrates

than has hitherto been supposed to exist. Here, again, there must be considerable sifting of evidence before a definite conclusion can be reached. Most naturalists will, however, be startled by Mr. Lowne's comparison of this invagination with that forming the neural canal in vertebrates, a comparison induced by the speculations of Gaskell and Patten that this canal represents an old alimentary tract. The near relationship between arthropods and vertebrates, required by this view, will need far more evidence than has yet been produced in its support; and most students will still prefer to regard a fly and a man as extreme specialisations in very different directions.

In the Diptera insect-metamorphosis reaches a high degree of specialisation. The organs of the larva are not transformed directly into those of the nymph and imago, but the larval tissues are dissolved within the pupa, while special invaginations (the imaginal discs) which had been present in the larva, form the pro-nymph. From this the nymph is developed, its external organs being evaginated, and it is in turn transformed into the imago. It follows from this that "not one single organ is common to the larva and the imago;" so "complete" is the metamorphosis, that some observers have regarded it as a case of alternation of generations, considering the nymph as a new embryo. To this somewhat wild suggestion, Mr. Lowne lends no support, but rightly explains it as an extreme case of ecdysis. He refers to the development of the Nemerteans in which the primitive ectoderm is ultimately cast, while the adult skin is formed by invagination from it, as an analogous case. It will be noted that Mr. Lowne speaks of the precursor of the imago as a "nymph," while he uses the term "pupa" for the dried up larva-skin within which the "nymph" is developed. This sense of the words is different from that adopted by entomologists generally.

The account given of the development of the legs in the nymph deserves special notice. At first there is a basal portion with two processes; these three parts are compared to the coxopodite, endopodite, and exopodite of a crustacean limb. The endopodite becomes five-jointed, and ultimately forms the tarsus. The exopodite lengthens and thickens, finally splitting longitudinally into a proximal (femur), and distal portion (tibia). The tarsus is, after the split, of course, attached to the tibia. This interpretation is ingenious, but it seems doubtful if the development can have the morphological meaning attached to it by Mr. Lowne. It is remarkable, however, that he and Jaworowski should almost simultaneously have compared respectively the limbs of flies and spiders with those of crustaceans.

Many of Mr. Lowne's theories will be combated; some will probably be refuted. He is careful, however, to state clearly the various opinions already put forward on disputed points, and to distinguish between the facts he describes and the hypotheses he builds upon them. A very complete bibliography adds greatly to the value of the work, and future investigators owe a debt of gratitude to our author for his full descriptions of his methods of study which will save them many fruitless experiments. G. H. C.

BIRDS: The Elements of Ornithology. By St. George Mivart, F.R.S. Pp. 306, figs. 170. London: R. H. Porter, and Dulau & Co., 1892. Price 6s.

"THIS small volume," we are told, "is put forward in the hope of supplying a want which its author has himself felt keenly. It is intended to supply, in a small compass, a general view of the Class of

Birds; together with such a knowledge of their structure, activities, geological and geographical relations, and classification, as may fit the student to enter upon a serious study of Ornithology." Dr. Mivart has set himself to accomplish this task in 306 small 8vo pages, which are illustrated by 170 woodcuts. The first half of the book contains a brief account, copiously illustrated—no less than 140 of the woodcuts are to be found in this "Introduction"—of the principal kinds of birds, and these illustrations, although rough, are effective enough and accurate. There are, perhaps, rather too many of the passerine birds, which often do not present salient differences that can be duly emphasised without the assistance of colour.

The anatomical part of the book is, in our opinion, not full enough for the beginner to whom Dr. Mivart addresses himself; it would have been better, we think, to give a more elementary account of a bird's structure at greater length, and with abundant illustration; or, on the other hand, the author might have contented himself with a description of those structures which vary most among birds, and which are therefore made use of for classificatory purposes. A person with a knowledge of comparative anatomy does not want such an abridged account as that which Dr. Mivart gives, for he has the excellent and elaborate treatises of Professor Fürbringer and Dr. Gadow; and for a beginner, with presumably no knowledge of anatomy, the chapter on structure would be decidedly too stiff. A very useful feature to ornithologists in the book before us is the list of structural characters that is given with each group in the classification which concludes the volume.

Les Plantes Alexitères de l'Amerique. By H. Bocquillon-Limousin. 8vo. Pp. 120. Paris, 1892.

Serpents, with their deadly bites, form one of the greatest scourges of the New World as well as Asia. In some parts the mortality is very great, as, for example, in Martinique, Mexico, Ecuador, Brazil, and the Argentine Republic. Happily for humanity, the flora of the country affords an antidote, and the object of this little book is to give an account of those tropical and sub-tropical American plants which experience has shown to be thus useful. The author evidently believes in a somewhat subtle relation between the flora and fauna, for, in concluding, he states his belief in the principle that the bite of a serpent must be counteracted by the action of a plant of the same country. As, however, he likewise affirms that the poison is nullified only by the adverse physiological action of the plant itself, not by that of the definite chemical principle contained, and also suggests elsewhere that the action of dried plants is very different from that of freshly-gathered plants, it is obvious why the serpent and the antidote must flourish in the same neighbourhood; otherwise, his statement seems based only upon the fact that the Condor of Ecuador always has recourse, when bitten, to the *Gonolobus condurango*, and similarly the Guaco heron of the Magdalena Valley to the *Mikania guaco*; while, according to authors, the latter plant is powerless against the *Trigonocephalus* of Martinique.

Under the botanical name of the plant is given the synonymy including native names. Then follow the habitat, a botanical description, and the therapeutic properties. In the more important cases, the account is a full one, including the minute anatomy of the stem, with sometimes illustrations of transverse

sections, a chemical analysis of the efficacious portion, its physiological action, the way to use it, and the dose. In several cases glucosides and alkaloids have been for the first time isolated, or thoroughly studied, *e.g.*, mikanine from *Mikania guaco*, condurangine from *Gonolobus condurango*, dorsténine, and others.

The physiological action of plants, or the chemical compounds produced in the course of their metabolism is a subject always deserving of study, both from a medical and scientific point of view, and it may be of use to have collected in one volume such a class as those treated of in "Les Plantes Alexitères de l'Amerique."

MINERALOGY. By F. H. Hatch, Ph.D., F.G.S. Pp. 124, figs. 115. London: Whittaker & Co., 1892. Price 3s. 6d.

THIS little volume, which belongs to Whittaker's Library of Popular Science, should prove useful as an elementary introduction to the scientific study of minerals, and in our opinion it differs from other English or American text-books of Mineralogy in precisely those features which make a book suitable for beginners.

The author refrains from describing many species or from entering into much detail, and the minerals selected for description are grouped according to those properties from which they derive their interest or importance. A brief introduction concerning the Characters of Minerals is succeeded by four short chapters on (1) Rock-forming Minerals, (2) Ores and Veinstones, (3) Salts and Useful Minerals, (4) Gems. This arrangement is evidently more attractive, and, we venture to think, more useful to a beginner than the customary method of describing one species after another in systematic order.

The book is of a quite elementary and popular character, and is written in the simple and lucid style which distinguished Dr. Hatch's "Introduction to Petrology." We can find nothing in this volume which should not be easily intelligible, except the explanation of cleavage on p. 36, of which the meaning is by no means apparent. Some of the figures are reproductions from photographs of specimens.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

British Association.

The recent meeting of the British Association held in Edinburgh was well-attended, the total number of tickets issued being 2,070. There were many distinguished foreign guests, and the number of associates would doubtless have been greater had not so many of the residents of Edinburgh been spending their vacation elsewhere at the time. In Natural Science, Geology was an especially prominent feature, Sir Archibald Geikie's Presidential Address dealing with the Doctrine of Uniformity and the Antiquity of the Earth in a popular manner, while the Sectional Addresses of Professors Lapworth and James Geikie were devoted to what we elsewhere (p. 481) term "The New Geology." Sir Archibald Geikie contended, from geological evidence, that the Earth must be much older than supposed by Lord Kelvin and other physicists.

Apart from the brilliant new ideas in Professor Lapworth's Address, the Geological Section advanced little beyond minor matters of detail. Mr. J. Lomas' demonstration of the scattering of fragments of rock from Ailsa Craig (in the middle of the Firth of Forth) over the Isle of Man, North Wales, and parts of Cheshire, was of great interest, the rock being so characteristic as to be always readily recognised. Messrs. Peach and Horne exhibited a supposed deep-sea ooze with radiolaria from the Silurian of the south of Scotland, and showed how the chert in contact with granite became completely recrystallised, consisting of large grains of quartz with inclusion of some garnets and mica. Mr. Clement Reid described seeds of some truly Arctic plants from the lowest deposit of certain silted-up lochs near Edinburgh; and Mr. E. T. Newton contributed a preliminary notice of the Elgin Dicynodonts and other reptiles, which have at last been rescued by the Geological Survey from the obscurity in which they have lain for six years at the Edinburgh Museum. The Committees of the Geological Section are not so active as they might be, and some of the Reports this year were far from satisfactory.

The success of the Biological Section was very marked in each of the three subdivisions. It is estimated that the aggregate number of zoological, botanical, and physiological papers was higher than that of all the other papers presented to the Association. Professor Rutherford's Address was an elaborate exposition and criticism of current views on the physiology of the colour-sense. A discussion on the possibility of uniting all the organisations devoted to Sea Fisheries in the British Isles may lead to important legislation. The Committees for the exploration of the West Indies and the Sandwich Isles reported good progress; and Mr. H. O. Forbes exhibited recently-discovered bird-remains from New Zealand. Dr. McCook gave an address on spiders, in which he described them as solitary animals; and in General Biology Dr. Beard discussed the relation of larval to adult animals. In the Botanical Department, foreign visitors came conspicuously to the front. Professor Goebel, of Munich, contributed a very important paper "on the simplest form of Mosses"—viz., *Buxbaumia*, long known to Bryologists as an extreme form, by reason of its remarkable sporogonium, and inconspicuous vegetative characters. Professor Goebel now announces that its antheridia are borne on the protonema. There is room for a difference of opinion as to the precise significance of this astonishing discovery, but the botanists present appeared to agree, on the whole, in

regarding it as a "degraded type." Side by side with this paper was read one by Professor Bower, in which, with admirable lucidity, he brought forward not only "modern instances" in support of his well-known views on the evolution of the Vascular Cryptogams, but drew support from a minute study, recently made, of fossil remains in the Botanical Department of the British Museum. Dr. Scott and Mr. Brebner made a solid contribution to plant anatomy, viz., an examination of cases of secondary thickening of the stems of certain Monocotyledons. This paper, indeed, like Mr. George Murray's "on a comparison of the marine floras of the warm Atlantic and Indian Ocean," seemed a little too solid to be easily digestible by a British Association audience. A paper was read by Miss Nina Layard on the arrangement of the buds in *Lemna*. Nothing very revolutionary came of it, and since the Secretaries of the Section did not emulate the zeal of their geographical colleagues when a lady "explorer" appears on the scene, no profound sensation was caused by the modest presentation of an unpretentious paper. On the whole, the botanical subsection showed no approach, even for a moment, to the giddiness associated in the public mind with these meetings.

Besides Professor James Geikie's Address on coast-lines, several other geological matters were submitted to the Geographical Section. Mr. John Thomson vividly described the eroding action of rain and streams in Formosa, and Mr. J. Y. Buchanan opened a discussion on the winding of rivers. There was a long discussion with the chemists on Oceanography; and the Prince of Monaco advocated the establishment of meteorological stations on the Azores and other Atlantic islands.

In his address to the Anthropological Section, Professor Macalister critically reviewed recent advances, and lamented the want of precision in anthropological works. The Committee for the Investigation of the Canadian Indians presented a good report; and there were important discussions on Criminal Anthropology, and on a proposed Ethnographic Survey of the British Isles. Dr. Garson's report on measurements of members of the Association at Cardiff was ungratefully received, being described as meaningless; and the exhibitor of some supposed pre-palæolithic flint implements from the North Downs of Kent met with little encouragement.

The General Committee of the Association voted the total sum of £1,000 to the various committees engaged in research. The grants to the Geological Section were small in amount, applied to the subjects of Erratic Blocks, Underground Waters, Geological Photographs, and some Palæontological Investigations. In Biology, the sum of £100 is granted for the maintenance of a table at the Naples Zoological Station, and £30 for another at Plymouth; £20 is devoted to the expenses of Mr. Sherborn's Index of the Genera and Species of Animals, and a similar amount for the study of Asphyxia; £100 is granted for the exploration of the Sandwich Islands, half that amount for further enquiries into the Zoology and Botany of the West India Islands, and £30 for dredging in the Irish Sea. In Geography, the Committee for the exploration of the Karakorum Mountains receives £50, and that for the investigation of the Climatology and Hydrography of Tropical Africa a similar sum; £10 is granted for the study of Scottish Place Names. In Anthropology the Committees on Ancient Remains in Abyssinia, the Tribes of N.W. Canada, and the Natives of India receive respectively £25, £100, and £10. The Corresponding Societies' Committee has £30, and the Anthropometric Laboratory £5.

THE organisation of the new University of Chicago is progressing well, and the first session will begin on October 1 next. The Director of the Biological Institute is Dr. Charles O. Whitman, who also holds the Professorship of Animal Morphology. Most of the Director's colleagues lately in the Clark University at Worcester (Mass.), accompany him in the new institution. The Professorship of Biology still remains vacant, but the Chairs of Anatomy and Comparative Neurology are held respectively by Dr. Franklin P. Mall and Dr. H. H. Donaldson. Dr. Georg Baur occupies the position of Assistant-Professor of Comparative Osteology and Palæontology.

In the August number of the *American Naturalist* there is an interesting account of the success of the Biological Department of the Leland Stanford Junior University, California. The number of students has been much in excess of anticipation, and the original equipment of the various laboratories proved far short of requirements. The new Marine Biological Station at Pacific Grove, Monterey Bay, is named the Hopkins Seaside Laboratory, the general furnishing having been provided by the liberality of Mr. Timothy Hopkins. The building was presented to the University by the Pacific Improvement Company and the inhabitants of Pacific Grove, and affords accommodation for about fifty students. Teachers and ordinary students pay a small fee, but original investigators are admitted to the laboratory free of charge.

The first part of the *Kansas University Quarterly*, dated July, 1892, has lately been received. The journal will be devoted to the results of original scientific researches undertaken by members of this Western University. The part to hand contains an illustrated account of the Cretaceous Pterodactyles and Mosasaurs of Kansas by Professor S. W. Williston, and some technical descriptions of insects.

Dr. J. S. Kingsley has been appointed Professor of Biology in Luft's College, College Hill, Mass., U.S.A.

Dr. Carl Berg has been appointed Director of the Museum of Buenos Ayres, in succession to the late Dr. H. Burmeister.

The latest publication of the Australian Museum, Sydney, is the second part of Mr. John Brazier's "Catalogue of the Marine Shells of Australia and Tasmania," comprising the Pteropoda. It is remarked that no mollusca of this class have yet been discovered off Tasmania and South or West Australia.

The American Museum of Natural History, New York, has lately acquired a series of mammalian teeth from the Laramie Formation. We are pleased to learn from the *American Naturalist* for August that the monopoly in collecting Laramie fossils attempted to be held by certain officers of the U.S. Geological Survey, is now to be destroyed. An expedition, under the direction of Dr. J. L. Wortman, will shortly be despatched by the American Museum to the Laramie region, for the collection of the Horned Dinosauria and associated fossils.

The Annual Report of the Trustees of the South African Museum, Cape Town, for 1891, has just reached England. In addition to their ordinary routine duties, the Curator (Mr. Roland Trimen, F.R.S.) and Assistant-Curator (Mr. L. Péringuey) are occupied with important researches in South African Entomology. The specimens acquired during the year are numerous and varied; but much difficulty is found in preserving the skins of the larger animals on account of insect pests, and the Museum has had the misfortune to lose some valuable diamonds and nuggets by theft. The extension of the premises, long contemplated, has hitherto been delayed by the unfavourable condition of the revenue, and the prospect that the new Museum might form part of the projected University Buildings. A further effort, however, is being made to erect an independent institution at an early date. At the end of the present year, the Museum will receive another important acquisition, in the form of a plaster cast of the skeleton of the great extinct reptile, *Pariasaurus baini*, from the South African Karoo, discovered by Professor H. G. Seeley, and now in the British Museum. This unique specimen was despatched last month by the Trustees of the British Museum to the Kimberley Exhibition, where it will be shown prior to its removal to Cape Town.

We are pleased to learn that the Director of the Natural History Departments of the British Museum, Professor Flower, has received the honour of knighthood, attaining the rank of K.C.B. Professor Huxley has also been honoured by election to the Privy Council.

The Museums Association meets next year in London, under the Presidency of Sir William Flower, when the possibility of establishing some closer connection between the British Museum and the Provincial Museums will doubtless be discussed. At its recent meeting in Manchester, the Association conferred with a deputation from the Libraries Association, in reference to the proposed emendation of the Acts under which Public Libraries and Museums are at present governed. An increase in the appropriation of public funds for the support of these institutions is desired.

In 1878, the Council of the Hastings and St. Leonards Philosophical Society published a small pamphlet on "The Natural History of Hastings and St. Leonards and the Vicinity," containing a complete list of the known Fauna and Flora. In 1883 and 1888, supplementary lists were issued, chiefly through the instrumentality of the Rev. E. N. Bloomfield, of Guestling. In 1889, the naturalists of the district held a meeting with some of the prominent citizens to discuss the possibility of founding a museum to illustrate both the Natural History and Antiquities of the town and neighbourhood. Subsequently the scheme received much support, becoming gradually realised under the auspices of the "Hastings Museum Association," and on August 16 last the nucleus of a Museum was formally opened in the upper rooms of the Brassey Institute. The Mayor of Hastings presided at the ceremony, and speeches were made by Mr. W. V. Crake (Hon. Sec. of the Association). Mr. Wilson Noble, M.P., Mr. Felix Joseph, and Mr. Smith Woodward (of the British Museum). According to the prospectus, the Natural History and Antiquarian Departments of the Museum are to be devoted exclusively to local specimens, arranged, as far as possible, for educational purposes; and a good beginning has certainly been made. The Committee, however, are already embarrassed with the inevitable pagan idols, foreign curios, and furniture blessed by contact with some famous man of the past; and unless they can devise some means of removing such "matter in the wrong place," their admirable intentions will soon be frustrated. The Art Exhibits also encroach much upon space that might be more profitably devoted to objects having local associations. A small Zoological Laboratory is to be added to the Museum, while courses of lectures and demonstrations are contemplated.

The Annual Report of the Conservator of the Museum of the Royal College of Surgeons of England, lately presented to the Museum Committee, records the completion of the two additional rooms that have so long been required for the expansion of the collection. Much progress has already been made in the re-arrangement necessitated by the addition. In the Department of Human and Comparative Anatomy the most important additions during the year are thirteen skeletons and twelve skulls of ancient Egyptians, probably dating back to 4000 B.C., presented by Dr. Flinders Petrie; and a large series of various skeletons and skulls of Vertebrata from the collection of the late Sir Victor Brooke, Bart., presented by his son.

An appendix to the volume of the Proceedings of the Royal Society of London just completed (vol. l.) is devoted to an interesting summary of the Society's second and third Charters and a historical notice of the Statutes. There is also a catalogue of the portraits, busts, and medals in the possession of the Society.

THE Imperial Academy of Sciences of Vienna has granted a subvention to Professor L. von Graaff, for the purpose of defraying the expenses of an expedition next Spring to the tropics, to collect material for the completion of the second volume of his Monograph of the Turbellaria.

THE Royal Scottish Geographical Society proceeds next month to the election of a new Secretary. Geographers will regret to learn of the retirement of Mr. Arthur Silva White on account of ill-health.

THE twenty-first anniversary of the Chester Society of Natural Science will be celebrated early this month. His Grace the Duke of Westminster is expected to preside, and several of the Society's honorary members will attend.

THE Annual Report of the Yorkshire Philosophical Society for 1891 records progress in the work of the Museum, but contributes little to original research. Except a brief record of two borings for water, the report contains only a statistical account of meteorological observations. We understand that the late Mr. William Reed bequeathed the sum of £600 to the Society, the interest to be devoted to acquisitions for the Museum.

THE Manchester Geological Society has just completed vol. xxi. of its *Transactions*. In the last part Mr. G. C. Greenwell discusses the possibility of discovering coal to the south of the Mendip Hills, and Mr. C. Roeder publishes some observations on marine shells in the Glacial Drift near Manchester.

MR. EDWARD F. PITTMAN, Chief Mining Surveyor, has been appointed Government Geologist of New South Wales in succession to the late Mr. C. S. Wilkinson. The annual report of the Geological Survey for 1891, lately received, is devoted almost exclusively to economic questions.

GEOLOGISTS will regret to learn of the death of Dr. Otomar Novàk, Professor of Geology in the Bohemian University of Prague, which took place on July 29, at the early age of 42 years. The Professor was occupied with the continuation of Barrande's great work on the Silurian fossils of Bohemia, specially investigating the corals.

OBSERVATIONS AND CORRESPONDENCE.

Dr. Weismann's Theory of Heredity.

I have received the following interesting observations from Dr. E. Sickenberger, Professor of Chemistry, Botany, and Materia Medica in the School of Medicine, Cairo, which readers interested in the subject may be glad to see:—"I have read with great interest your publication upon Dr. Weismann's theory, and I am very glad to see that your conclusions are entirely in accordance with my own observations. I believe, if Weismann had had more special knowledge of botanical matters, he would not have undertaken to apply his theory to plants. You have completely refuted by your explanations his assertion that a continuity of germ-plasm exists in a certain series of somatic cells only. I cannot admit any essential difference of characters between species and varieties, such being only due to a greater or less degree of qualities acquired by the influence of external conditions during a longer or shorter period. Hybridisation comes in, in a secondary way, mixing those acquired qualities with another series. Hereditary persistence is, I believe, nothing else than the maintenance of those acquired qualities under the power of inertia (Gesetz der Trägheit). The more any acquired qualites have changed the anatomical and chemical constitution of a plant, so much the more will those qualities be persistent. If Weismann says 'acquired characters cannot be transmitted at all,' every day in Egypt contradicts this opinion, and the fact that 'species of plants in Egypt have remained unchanged for thousands of years,' only proves that the climatal conditions of Egypt have remained all the time unchanged. On the other hand, seeds of *Gignut* [?], a variety of *Cannabis sativa* from Europe, produce the true *Cannabis sativa* yarn by the third generation. Black mustard, *Brassica* (*Sinapis*) *nigra* is transformed in the second generation into the endemic *B. bracteolata*, Fisch. et Mey. [Plante particulière à l'Egypte (Asch. et Schweinf.)], and the thick-rooted celery assumes in the first year the much foliated form with a thin root-stock, like the summer spontaneous form of Egypt. I have myself had the opportunity of substantiating these facts several times. On the other hand, it must be proved that external influences act in some other way upon the germ-plasm than upon the somato-plasm—all action in nature being based upon the universal physical and chemical forces—so that really the constitution of the pretended germ-plasm is different from that of the somato-plasm."

George Henslow.

TO CORRESPONDENTS.

All communications for the Editor *to be addressed to the* Editorial Offices, 67–69 *Chancery Lane, London, W.C.*

J. Wilson (*Stamford*).—The Conchological Society's List of British Land and Freshwater Mollusca has already appeared: see *Journ. Conchology* for April, 1892.

W. H. Corder (*Lancaster*).—The Darwin collection of Cirripedia is now in the British Museum, but we believe that all the duplicates are to be seen in the Liverpool Museum. The "*Coronulites diadema*" to which you refer is also in the British Museum. The Parkinson sale took place in 1826, and the specimens were widely distributed; this specimen, however, finally reached the national collection through a Bury-St.-Edmunds collector.

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